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From May 10 *to June* 21, 1894.

VOL. LVI.

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PROCEEDINGS

OF

THE ROYAL SOCIETY.

May 10, 1894.

The LORD KELVIN, D.C.L., LL.D., President, followed by Sir JOHN EVANS, K.C.B., D.C.L., LL.D., Vice-President and Treasurer, in the Chair.

Professor Dmitri Ivanovitch Mendeleeff, who was elected a Foreign Member in 1882, signed the obligation in the Charter Book and was admitted into the Society.

Mr. Benjamin Neeve Peach (elected 1892) was admitted into the Society.

A List of the Presents received was laid on the table, and thanks ordered for them.

In pursuance of the Statutes, the names of the Candidates recommended for election into the Society were read from the Chair as follows:—

Bateson, William, M.A.
Boulenger, George Albert.
Bradford, John Rose, M.D.
Callendar, Professor Hugh Longbourne.
Cheyne, Professor William Watson, M.B., F.R.C.S.
Froude, Robert Edmund.
Hill, Professor M. J. M., M.A., D.Sc.
Jones, Professor John Viriamu, M.A., B.Sc.
Love, Augustus Edward Hough, M.A.
Lydekker, Richard, B.A.
Penrose, Francis Cranmer, M.A., F.R.A.S.
Scott, Dukinfield Henry, M.A., F.L.S.
Smith, Rev. Frederick John, M.A.
Swan, Joseph Wilson, M.A., F.I.C.
Veley, Victor Herbert, M.A., F.C.S.

The following Papers were read:—

I. "The Composition of Atmospheres which Extinguish Flame."

By Frank Clowes, D.Sc., Lond., Professor of Chemistry, University College, Nottingham. Communicated by Professor Armstrong, F.R.S. Received March 14, 1894.

1. *Introductory Remarks.*

A study of the experiments which have been made to determine the composition of atmospheres, which act extinctively upon flame, shows that in many cases the atmosphere under examination was in contact with water. The solvent action of water on the carbon dioxide present seems in such cases likely to disturb the composition of the mixture. In other cases, only the proportion of oxygen in the extinctive atmosphere was noted, and the nature of the diluent gas or gases was not taken into consideration. Experiments were also limited to the flames of a few combustible substances, or where a wider range of different flames were tried, the results reported were only of an approximate and relative nature.

The experimental work, the results of which are summarized in this communication, was undertaken in order to supplement the deficiencies referred to above, with the view of drawing further generalisations, and of furnishing support to those already drawn from previous experiments.

2. *Method of Experimenting.*

The mixtures of air with the extinctive gas were made in a glass cylinder, which was closed by a ground glass plate.

A measured quantity of water, equal in volume to the percentage of extinctive gas to be mixed with the air, was first poured into the glass cylinder. The cylinder was then closed by the plate and inverted in a vessel of water. A light xylonite ball of known volume was then passed up, and the extinctive gas was introduced in sufficient quantity to fill the cylinder. The cylinder was then closed and its contents were mixed by the movement of the ball.

In order to test the accuracy with which any desired mixture of gases could be prepared by this method, two mixtures of air with carbon dioxide were submitted to analysis. They furnished respectively 9·8 instead of 10 per cent., and 69·7 instead of 70 per cent. of carbon dioxide.

The experimental flames used were 0·75 in. in height and were gradually lowered into the cylinder, the top of which was finally covered by the plate. The gases were burnt from a platinum jet 1 mm. in diameter.

The gaseous mixture was considered to be in extinctive proportions if the flame was extinguished during its downward passage, or immediately upon attaining its lowest position in the cylinder. The mixture was considered to contain the *minimum necessary quantity* of extinctive gas, when another mixture containing 1 per cent. less of the extinctive gas allowed the flame to continue burning in it for a few seconds only.

The limiting differences between the results of repeated trials corresponded to 1 per cent. of the extinctive gas in the air.

This minimum necessary percentage of extinctive gas is recorded below in tabulated form.

It was considered necessary to take the immediate extinction of the flame as the criterion of extinctive power, since the composition of the atmosphere was rapidly affected by the combustion of the flame.

3. *Influence of the Size of the Flame.*

As a matter of convenience, the flames were, in all cases, set to a height of 0·75 inch. But a series of experiments was undertaken with the same flame of varying size, in order to ascertain if the proportion of extinctive gas necessary to extinguish the flame varied with the size of the flame.

The results of these experiments with flames of hydrogen and alcohol, varying from 0·4 in. to 1·5 in. in height, show that the varying dimensions of the flame, within the wide limits included in the trials, are without influence on the proportion of carbon dioxide in the air necessary to produce extinction.

4. *Method of Preparation of Gases Used.*

The carbon dioxide employed for the experiments was prepared in the usual way by the action of diluted hydrochloric acid upon marble. It was washed with water, and was proved to be practically free from air.

The nitrogen was prepared by heating an aqueous solution containing potassium nitrite, ammonium chloride, and potassium dichromate. An analysis of the resulting gas proved that it contained 99·7 per cent. of nitrogen.

5 *Results obtained by the Experiments.*

In the following table the number entered is the average of numerous closely concordant experimental results. The percentage volume of nitrogen in air is taken as 21.

Combustible substance burnt.	Extinctive proportion of carbon dioxide added to air.			Extinctive proportion of nitrogen added to air.		
	Percentage added.	Percentage composition of mixture. O	: $(N+CO_2)$.	Percentage added.	Percentage composition of mixture. O	: N
Alcohol, absolute....................	14	18·1	81·9	21	16·6	83·4
Alcohol, methylated..................	13	18·3	81·7	18	17·2	82·8
Paraffin, ordinary lamp oil	15	17·9	82·1	23	16·2	83·8
Colza oil with equal volume of petroleum	16	17·6	82·4	22	16·4	83·6
Candle	14	18·1	81·9	22	16·4	83·6
Hydrogen............................	58	8·8	91·2	70	6·3	93·7
Carbon monoxide	24	16·0	84·0	28	15·1	84·9
Methane.............................	10	18·9	81·1	17	17·4	82·6
Ethylene............................	26	15·5	84·8	37	13·2	86·8
Coal-gas	33	14·1	85·9	46	11·3	88·7

Characteristic differences were observed between the behaviour of wick-fed flames and that of gas-fed flames when they were introduced into an atmosphere which extinguished them. The wick-fed flames gradually diminished in size until they vanished. The gas-fed flames, on the other hand, gradually increased in size, becoming pale and apparently lower in temperature, and then suddenly expired. The extinction of the flame is apparently due in both cases to the lowering of its temperature. This primary cause, however, seems to operate directly in the case of the gas-fed flame, whilst in the case of the wick-fed flame it operates by gradually reducing the amount of combustible gas and vapour produced, and leads ultimately to the flame dying from lack of combustible material. The large expansion of the gas-fed flame is evidently due to an attempt to obtain the necessary supply of oxygen in the diluted atmosphere by increasing its own surface.

6. *Theoretical Considerations.*

The following deductions seem to be warranted by the results arrived at in these experiments:—

1. That the extinction of a flame is not determined only by the *proportion* which the inert gas bears to the oxygen of the atmosphere into which it is introduced, but that the *nature* of the inert gas present also influences the result.
2. That carbon dioxide uniformly exerts a more powerful extinctive effect upon flame than nitrogen does.
3. That there is a remarkable uniformity in the proportions of inert gas which must be mingled with air in order to just extinguish wick-fed flames.
4. That this uniformity does not apply to the flames of combustible gases burnt from a jet.
5. That the flames of gases burnt from a jet show no simple relation, as regards the proportion of oxygen present in the extinctive atmosphere, to the relative proportions of oxygen required for their complete combustion.

With regard to the superior extinctive power of carbon dioxide over that of nitrogen, it has been stated that the greater the density of an inert gas which is introduced into air, the less will be the quantity which suffices to arrest combustion. Waldie suggests that this is due to the cooling effect produced upon the flame by the rapidity of diffusion of its heated products increasing as the surrounding atmosphere increases in density. But it is probable that carbon dioxide also surpasses nitrogen in its extinctive effect upon flame in virtue of its higher specific heat, and because of its slower movement owing to its high molecular weight and density. When

the heavy gas is mingled with the air, it adds to the density of the mixture, and renders the atmosphere more sluggish in its movement towards the flame to supply the necessary oxygen.

It had been anticipated that in the presence of the hydrogen flame, and possibly of other flames, carbon dioxide would have suffered partial deoxidation, as it is well known to do in the presence of burning magnesium vapour. No such action appeared to occur, else the above relation between the extinctive powers of carbon dioxide and nitrogen could not well exist.

The cause of the comparative uniformity of the proportion of extinctive gas required for wick-fed flames has been already hinted at. The flames are starved of combustible nutriment by the lowering of the temperature of the flame. This cause seems to operate with strikingly similar results upon the different solid and liquid combustibles.

The cause of the want of conformity to theoretical considerations in the case of the gaseous flames fed from jets is not at once apparent.

It is of practical interest to note that the introduction of a minimum of 15 per cent. of carbon dioxide into air is necessary to cause it to extinguish ordinary wick-fed flames, the oxygen being reduced by this admixture from the normal proportion of 21 per cent. to 18 per cent. For the extinction of a coal-gas flame, however, the addition of 33 per cent. of carbon dioxide is necessary, and the oxygen being thus reduced to 14 per cent. The hydrogen flame has far greater vitality, requiring the admixture of 58 per cent. of carbon dioxide with air, and the consequent reduction of the oxygen to 8·8 per cent., before it suffers extinction. This fact is of great importance, since it shows that the hydrogen flame in the composite miner's safety lamp ('Roy. Soc. Proc.,' vol. 52, p. 486) may be used as an auxiliary to prevent the loss of flame when the lamp is being carried through mine-air containing large proportions of carbon dioxide.

I have to thank one of my senior students, Martin E. Feilmann, B.Sc., for conducting much of the experimental work involved in this research.

[*April 28th*, 1894.—Recent experiments seem to prove that a rabbit can breathe with impunity, for at least an hour, air containing 25 per cent. of admixed carbon dioxide (J. R. Wilson, 'American Journal of Pharmacy,' 50, No. 12). If this is the case, the extinction of an ordinary flame does not prove the surrounding atmosphere to be irrespirable. The introduction of 15 per cent. of this gas extinguishes a flame, whilst the air seems to be still respirable, even after it has been mingled with an additional 10 per cent. of carbon dioxide.—F. C.]

II. "Preliminary Report on the Results obtained with the Prismatic Camera during the Total Eclipse of the Sun, April 16, 1893." By J. NORMAN LOCKYER, C.B., F.R.S. Received February 22, 1894.

(Abstract.)

During the total eclipse of 1871 observations were made by Respighi and the author with a spectroscope deprived of its collimator, and a series of rings was seen corresponding to the different rays emitted by the corona and prominences. A similar instrument, arranged for photography, was employed during several succeeding eclipses, but the photographs were on so small a scale that none of the results came up to the expectations raised by the observations of 1871. As the Solar Physics Committee is now in possession of a prismatic camera of 6 inches aperture, the prism having a refracting angle of 45°, it was determined to employ it during the eclipse of 1893. The instrument was placed at the disposal of the Eclipse Committee by the Solar Physics Committee, and was entrusted to Mr. Fowler, who took the photographs at the African station.

It also seemed desirable that a series of similar photographs should be taken at another point on the line of totality, even though an equally efficient instrument were not available. A spectroscope with two 3-inch prisms of 60°, used in conjunction with a siderostat, accordingly formed part of the equipment of the expedition to Brazil, and was placed in charge of Mr. Shackleton.

The present preliminary report is intended to indicate the kind of results obtained, and some of the photographs are reproduced for the information of those specially interested, as it will be some time before the complete reductions are ready for publication.

At the African station 30 plates were exposed, 15 during totality, and the remainder in the five minutes before and after totality. In Brazil 17 plates were exposed during totality, and 7 out of totality.

The most conspicuous lines, or rather portions of circles, seen in the photographs taken during totality, are the H and K lines of calcium, and in these rays the images of the various prominences are very clearly outlined.

The lines of hydrogen, extending far into the ultra violet, are also very prominent, and numerous other lines are seen in addition.

Isochromatic plates were used for some of the exposures, and on some of these the ring formed by the characteristic line of the coronal spectrum (1474 K) is clearly depicted, especially in the Brazilian photographs. A comparison with the photographic records of the corona shows that the prismatic camera has picked out the brightest

parts of the corona in this way. All the photographs show a bright continuous spectrum from the inner corona.

Some of the plates taken out of totality show numerous bright lines at the cusps of the crescent of the sun then visible, chief among them being the lines of hydrogen and the H and K lines of calcium; others, farther removed from the second and third contacts, show only the Fraunhofer lines.

III. "Researches on Modern Explosives.—Preliminary Communication." By WILLIAM MACNAB, F.I.C., F.C.S., and E. RISTORI, Ass. M. Inst. C.E., F.R.A.S. Communicated by Professor RAMSAY, F.R.S. Received February 28, 1894.

During the last two years we have carried out a long series of experiments with explosive compounds for the purpose of studying chemical reactions at high temperatures and pressures, and of elucidating certain thermal constants relating chiefly to the specific heat of gases under such conditions.

For these experiments we have principally used nitro-glycerin, nitro-cellulose, and several combinations of these two bodies which are used as smokeless gunpowders, for the reason that such modern explosives offer the advantage of not only presenting comparatively simple chemical reactions, owing to the absence of solid residue, but also of enabling considerable variations to be made in their composition so as to vary the proportions of the elements reacting.

We also expected that the results which we obtained would make a small contribution to the knowledge of explosives in general, following up the lines indicated by the published work of Noble and Abel, Berthelot, Sarrau, Vieille, and others.

In this preliminary communication we propose chiefly to indicate the results obtained in the measurement of the heat evolved by explosion, and of the quantity and composition of the gases produced by this metamorphosis.

We have also made considerable progress towards the determination of the actual temperature of explosion, and we have succeeded in recording these high temperatures by photographic means, but, as this work is not yet completed, we shall not further refer to it in this paper, but we hope it will make the subject of another communication at an early date.

These modern explosives, and especially the smokeless powders, have assumed of late such importance that it may be of general interest to give here a brief sketch of their development.

About thirty years ago experiments were made in Austria with the

object of using gun-cotton for the charges of rifle ammunition, but no success was obtained, and the matter dropped.

Other explosives, consisting principally of nitro-lignin or nitro-cellulose, not gelatinised, and mixed with nitrates or other substances, were afterwards invented and adopted for sporting guns successfully, and have been largely sold in the market under different well-known names. These explosives, however, were not found suitable for the charges of rifles and guns.

Further development in the science of artillery, and a better knowledge of the action of explosives, encouraged further researches for the production of new propelling agents for rifles and guns, and these researches have been so far successful that in a few years several new powders have been produced, each one of which is far superior to black gunpowder.

The new explosives now in use contain nitro-cellulose as one of their principal elements; some of them contain also nitro-glycerin in more or less proportion; the nitro-cellulose, by solution in nitro-glycerin, acetone, or other suitable solvent, is gelatinised, and by mechanical means the explosive compound is compressed and squirted into cords, or rolled into sheets, and then cut into strips or grains of suitable size for the different firearms.

The great secret of all these modern explosives seems to be that by the above means they are made into a solid substance, thus avoiding any porosity, and it appears probable that by doing so even the most powerful explosive can be mastered, so that, burning regularly from the surface, the rate of combustion can be controlled so as to avoid detonation.

This constitutes the most striking feature of the modern smokeless gunpowders, especially of those containing nitro-glycerin. If certain sized cubes, strips or cords of such powders are fired in a certain gun, and the length of this gun does not allow of sufficient time during the travel of the shot, for the explosive to be entirely consumed, the unburnt residue of the charge will be found to be of the same shape, whether cubes, strips or cords, only reduced in size; thus proving the most perfect surface combustion of these explosives.

It is thus possible to determine accurately what quantity of explosive, and what surface of combustion for the same, will be required, in order to obtain certain results in a certain gun, thus avoiding waste of powder.

This property of modern smokeless powder was illustrated on the occasion of a disastrous fire which occurred in May, 1890, at the factory of Avigliana, Italy, where large quantities of the explosive called ballistite were manufactured for the Government. In one building twelve tons of this explosive were collected, and various operations of manufacture were performed. By accident some of it

took fire, and the whole quantity was burnt in a few seconds. Though this powder was made of such powerful explosives as nitro-glycerin and nitro-cellulose, and though the amount was so large that, had it been black powder, it would have caused destruction for many miles around, still there was no explosion of any kind; none of the machinery was in any way damaged, and the wood was barely charred.

The explosives used in these experiments can be divided into three classes:—

1. Those consisting of nitro-lignin or nitro-cellulose (not gelatinised), mixed, or impregnated with a suitable nitrate, and mixed with colouring matters and some other substances for the purpose of retarding the rate of combustion. We have taken as samples of this class the EC and the SS powders now commonly used in sporting guns (the EC consisting principally of nitro-cellulose mixed with barium nitrate and a small proportion of camphor, the SS powder consisting of nitro-lignin mixed with barium nitrate and nitro-benzene).

2. Those consisting of purified nitro-lignin or nitro-cellulose gelatinised by a suitable process, and with or without the addition of nitro-benzene or other suitable nitrates.

As sample of this class we have taken the BN powder manufactured by the French Government, and also the Rifleite and the Troisdorf powder, which are now commonly used for small arms ammunition. (The BN consists mainly of gelatinised nitro-cellulose; the Troisdorf also consists of gelatinised nitro-cellulose, but is coated with graphite. Rifleite is also made with gelatinised nitro-cellulose, with the addition, however, of a certain proportion of nitro-benzene).

3. Those consisting of nitro-cellulose combined with nitro-glycerin, with the addition of aniline, camphor, vaseline, or other kindred substances. To this class belong cordite and ballistite.

Cordite contains 58 per cent. of nitro-glycerin, 37 per cent. of gun-cotton, and 5 per cent. of vaseline.

Ballistite of Italian manufacture contains equal parts of nitro-cellulose and nitro-glycerin, with the addition of $\frac{1}{2}$ per cent. of aniline.

Ballistite of German manufacture contains a slightly higher percentage of nitro-cellulose, and is coated with graphite.

Besides, for the purpose of these experiments, a series of samples of ballistite were specially made containing nitro-glycerin and nitro-cellulose in various proportions.

The experiments were carried out in two closed vessels of different dimensions and construction—a large one capable of standing high pressures, and a small one for calorimetric work.

The large one consists of a steel cylinder of great thickness, closed at both ends by conical screw-plugs. One plug is provided with a crusher-gauge of the well-known pattern by which the compression of a small cylinder of copper serves to measure the pressure developed. The other plug is provided with an insulated conical core, by means of which an electric current can be passed for the purpose of firing the charge. A small hole on the side of the cylinder, bushed with iridium-platinum, and closed by a coned screw-plug, serves to control the escape of the gases produced by the explosion.

The capacity of the chamber was carefully measured, and was found to be 247·6 c.c.

The small vessel is of the same pattern as used by Berthelot, and was made by Golaz, of Paris. It has given great satisfaction, and is in excellent order, although it has been used for more than two hundred explosions.

This bomb, which is made of mild steel and is cylindrical in shape, consists essentially of three parts—a bowl, a conical lid which is accurately ground into the bowl, and a tightening cap which screws on to the bowl over the lid.

There is a small hole in the lid provided with a delivery tube, which can be opened and closed by means of a finely-threaded conical plug. There is also an insulated platinum cone inserted from underneath in the lid, which admits of the charge in the bomb being fired by a platinum wire heated to redness by electricity.

From the lid depend platinum supports which carry a platinum capsule, in which the explosive is placed and suspended in the middle of the chamber.

The capacity of this bomb is 488 c.c., and the total weight, including a small stand, when ready for immersion in the calorimeter, is 5633·28 grams.

The calorimeter is made of thin sheet brass, and a helicoidal stirrer of the same metal (Berthelot's pattern), driven by a small electromotor during the experiment, serves to thoroughly mix the water.

The calorimeter stood in the centre of an annular water-jacket covered with felt. The quantity of water used in the calorimeter each time was 2,500 grams, and the equivalent in water of the bomb, stirrer, and calorimeter, due allowance having been made for the different specific heats of the different metals, is 623·4 grams.

The different thermometers employed were specially made by Casella, capable of being read to 0·005 of a degree centigrade, and the weights of their stems, bulbs, and mercury were known.

Various experiments were made in the large vessel, especially for the purpose of determining the pressure of the gases under different densities of charge.

These trials were carried out in a field, the bomb being lowered into a hole in the ground before firing.

Various difficulties were encountered, and in one experiment considerable damage was done by the heated gases effecting their escape at the moment of explosion, and "washing away" part of the thread of one of the screw plugs.

With a density of loading of $\Delta = 0{\cdot}1$, *i.e.*, with a charge of 24·76 grams, the average of the pressures measured was 6·3 tons per square inch; with density $\Delta = 0{\cdot}2$ the pressure rose to 15 tons, and with $\Delta = 0{\cdot}3$ the pressure increased to 25 tons. These results are very similar to those published by Sir A. Noble, F.R.S.

With the small bomb were ascertained the amount of heat generated by the explosion, the volume and composition of the permanent gases resulting, and the quantity of aqueous vapour produced.

As most of the explosives contained no mineral matter beyond a trifling percentage of "ash," it has been possible to analyse them in this way, the products of explosion when calculated from the analysis and volume of permanent gas and aqueous vapour agreeing closely with the weight of matter in the bomb before firing.

A few of the explosives left a carbonaceous or mineral residue; but these will be specially noticed further on in connexion with the tables of the results.

The heat evolved was measured by placing the bomb containing the charge of explosive in the calorimeter containing 2500 grams of water, and it was arranged that the temperature of the air, the water jacket, and the calorimeter closely approximated each other. The stirrer was set in motion, and the thermometer in the calorimeter was read with a kathetometer. Observations of the temperatures were made every minute for the five minutes preceding the firing of the charge, and continued at intervals of a minute until the maximum was reached, and for five minutes longer. The correction for loss of heat due to radiation of heat during the experiments amounted in general to about 0·01 of a degree. The increase in temperature varied from about 1° to 2½° C. according to the charge and explosive used.

The gas generated by the explosion was passed through weighed drying tubes connected with the valve on the lid of the vessel, and then collected and measured in a calibrated glass cylinder over mercury. The reading of the barometer and thermometer was noted, and the volume reduced to 0° C. and 760 mm.

The water was determined by immersing the bomb in a vessel containing boiling water. A three-way glass stop-cock intervened between the valve of the bomb and the drying tubes, and the other end of the drying apparatus was connected with a water vacuum pump.

The other branch of the three-way tap was connected with a separate drying apparatus. When the water surrounding the bomb was boiling, by starting the vacuum pump the steam and water were drawn into the absorbing apparatus; after a good vacuum had been made in the bomb the three-way tap was turned so that dry air rushed in, then connexion was made with the drying apparatus, the bomb again exhausted, and so on, alternately, until (as experience showed) all the water had been removed from the bomb and collected in the drying tubes, which were then weighed. The weights of water thus obtained were calculated for comparison into volumes of H_2O gas at 0° C. and 760 mm.

The analyses of gas were carried out in duplicate in Dittmar's apparatus as improved by Lennox.

In most of the experiments the bomb, previous to firing, was exhausted, and the amount of residual pressure, varying from 24 to 40 mm., noted on closing it. The amount of air corresponding to these pressures left in the bomb has the effect of increasing the heat generated by a small quantity amounting to 5 to 7 calories. This quantity being within the limits of error of the calorimetric observation no correction was made for the same, but the quantity of residual air was taken into account when comparing the weights of the products found with the weight of the explosive used. Thus in Tables I and II the volumes of gas of the given composition and of aqueous vapour were obtained from the given weight of explosive increased by the weight of the air corresponding to the vacuum indicated.

When firing in an exhausted bomb it was found necessary to have the explosive surrounding the firing wire in comparatively small pieces in order to ensure ignition of the whole charge.

Table I gives the principal results obtained with the several gunpowders above mentioned, Tables II and III give the results obtained with samples of ballistite made with different proportions of the component parts, Table IV indicates the effect of firing different weights of the same explosive in a closed vessel from which the air has *not* been exhausted, and Table V gives the original elementary composition of several explosives compared with the products of combustion, both being represented as weights.

With the exception of the results given in Table IV, all the others were obtained from the firing of 4 grams of the explosive.

In Tables I and II we have expressed the results of firing some powders now in use as well as certain specially prepared powders, so as to show the quantity of heat and the volumes and analyses of the gases produced, and have in the column headed "Coefficient of potential energy," given figures which serve as a measure of comparison of the power of the several explosives. These figures are

Table I.—Indicating the Quantity of Heat, also the Volume and Analysis of the Gas developed per gram with different Sporting and Military Smokeless Powders now in use.

Name of explosive.	Calories per gram.	Permanent gases.	Aqueous vapour.	Total volume of gas calculated at 0° and 760 mm.	Per cent. composition of permanent gases.					Coefficient of potential energy.
					CO_2.	CO.	CH_4.	H.	N	
		C.c. per gram.	C.c. per gram.	C.c. per gram.						
EC powder } English	800	420	154	574	22·9	40·6	0·5	15·5	20·5	459
SS sporting powder } English	790	584	150	734	18·2	45·4	0·7	20·0	15·7	586
Troisdorf, German	913	700	195	895	18·7	47·9	0·8	17·4	15·2	844
Rifleite, English	864	766	159	925	14·2	50·1	0·3	20·5	14·9	799
BN, French	833	738	168	906	13·2	53·1	0·7	19·4	13·6	755
Cordite, English manufacture	1253	647	235	882	24·9	40·3	0·7	14·8	19·3	1105
Ballistite, German manufacture	1291	591	231	822	33·1	35·4	0·5	10·1	20·9	1061
Ballistite, Italian and Spanish manufacture	1317	581	245	826	35·9	32·6	0·3	9·0	22·2	1088

Table II.—Indicating the Quantity of Heat, also the Volume and Analysis of the Gas developed per gram with Nitro-glycerin, Nitro-cellulose, and with several different Combinations of these two Explosives made at Ardeer Factory.

Composition of explosive.	Calories per gram.	Permanent gas.	Aqueous vapour.	Total volume of gas calculated at 0° and 760 mm.	Per cent. composition of permanent gases.						Coefficient of potential energy.
					CO_2.	CO.	CH_4.	O.	H.	N.	
		C.c. per gram.	C.c. per gram.	C.c. per gram.							
A. Nitro-glycerin	1652	464	257	741	63·0	—	—	4·0	—	33·0	1224
B. Nitro-cellulose (nitrogen = 13·30 per cent.)	1061	673	203	876	22·3	45·4	0·5	—	14·9	16·9	929
C. {50 per cent. nitro-cellulose (N = 12·24 per cent.) 50 per cent. nitro-glycerin ......}	1349	568	249	817	36·5	32·5	0·2	—	8·4	22·4	1102
D. {50 per cent. nitro-cellulose (N = 13·3 per cent.) 50 per cent. nitro-glycerin .....}	1410	550	247	797	41 8	27·5	0·0	—	6·0	24 7	1124
E. {80 per cent. nitro-cellulose (N = 12·24 per cent.) 20 per cent. nitro-glycerin .....}	1062	675	226	901	21 7	45·4	0·1	—	15·7	17·1	957
F. {80 per cent. nitro-cellulose (N = 13·30 per cent.) 20 per cent. nitro-glycerin......}	1159	637	227	864	26 6	40·8	0 1	—	12·0	20·5	1001
G. {35 per cent. nitro-cellulose (N = 13·30 per cent.) 5 per cent. vaseline .... 60 „ „ nitro-glycerin .....}	1280	627	236	863	26·7	39·8	0·5	—	12·8	20·2	1105

the products of the number of calories by the volumes of gas, the last three figures being suppressed in order to simplify the results.

In the case of EC and SS a certain amount of mineral residue was left, but this was not determined.

Troisdorf leaves a slight, and Rifleite and BN a considerable, carbonaceous residue, part of it adhering so tenaciously to the bomb that an exact determination was not made.

In the other experiments recorded in Tables I and II the degree of accuracy of the results may be gauged by the fact that the average weight of the products of explosion, calculated from the results found, amounts to 99·7 per cent. of the weight of the explosive fired, the extreme limits being 100·5 and 98·9 per cent.

In Table II the comparison of the pairs of results from explosives made with lower and more highly nitrated nitro-cellulose shows that the use of the highly nitrated cellulose increases the quantity of heat developed, and diminishes the volume of gas. The composition

Table III.—Showing the Heat developed by Explosives containing Nitro-glycerin and Nitro-cellulose in different proportions.

Composition of explosives.			Calories per gram.
Nitro-cellulose (N = 13·3 per cent.)		Nitro-glycerin.	
100 per cent. (dry pulp)		0	1061
100 " " (gelatinised)		0	922
90 " "		10 per cent.	1044
80 " "		20 " "	1159
70 " "		30 " "	1267
60 " "		40 " "	1347
50 " "		50 " "	1410
40 " "		60 " "	1467
0 " "		100 " "	1652
Nitro-cellulose (N = 12·24 per cent.)		Nitro-glycerin.	
80 per cent.		20 per cent.	1062
60 " "		40 " "	1288
50 " "		50 " "	1349
40 " "		60 " "	1405
Nitro-cellulose (N = 13·3 per cent.)	Vaseline.	Nitro-glycerin.	
55 per cent.	5 per cent.	40 per cent.	1134
35 " "	5 " "	60 " "	1280

of the permanent gases is also altered, as might be expected, there being an increase in carbonic acid and decrease in carbonic oxide and hydrogen.

The similarity in the volumes of gas produced and the composition of the permanent gases in the case of experiments F and G is worthy of note when the great difference in the original component ingredients of the explosives is borne in mind.

Table III shows clearly the increase of heat due to increased percentage of nitro-glycerin, as well as the difference of heat evolved from explosives containing nitro-cellulose of different degrees of nitration.

The diminution in quantity of heat (about 200 calories) which the replacement of 5 per cent. of nitro-cellulose by vaseline makes is also very striking.

Table IV.—Showing the Heat developed and the Analysis of the Permanent Gas produced in a closed Vessel from which the Air has not been exhausted—the Explosive being in every case Ballistite of Italian Manufacture.

Charge.	Calories per gram.	Analysis of the permanent gas.			
		CO_2.	CO.	H.	N.
2 grams	1587	37·0	17·6	3·2	42·2
3 "	1485	36·4	22·0	4·6	37·0
4 "	1446	36·2	24·6	6·1	33·1
5 "	1415	36·2	26·0	7·2	30·6
6 "	1380	36·3	27·0	7·9	28·6

Traces of CH_4 were found, but in this series of experiments the quantity of this gas was not determined.

Table IV shows the part played by the oxygen of the air in the bomb; when a smaller proportion of explosive in comparison with the air is present the combustion is more complete, and the heat evolved is greater, and the composition of the gases is correspondingly modified.

In Table V the elementary percentage composition of some of the explosives, along with the percentage composition of the products of explosion by weight, is given.

The composition of the samples has been calculated from the "bomb" analyses; as an example, one of the explosives and its decomposition may be represented approximately by the following equation.

We have assumed the nitro-cellulose to consist of a mixture of di-

Table V.—Showing the original Composition and Metamorphosis of Nitro-cellulose, Nitro-glycerin, and of several Gunpowders made by Combinations of these two Explosives.

Nature and description of explosive.	Per cent. composition by weight.				Per cent. products of combustion by weight.						
	Carbon, C.	Oxygen, O.	Hydrogen, H.	Nitrogen, N.	Carbonic acid, CO_2.	Carbonic oxide, CO.	Marsh gas, CH_4.	Oxygen, O.	Hydrogen, H.	Nitrogen, N.	Water H_2O.
A. Nitro-glycerin	15·7	63·0	2·3	18·8	57·6	—	—	2·7	—	18·8	20·7
B. Nitro-cellulose (nitrogen = 13·3)	24·58	57·68	2·73	13·6	29·27	38·52	0·24	—	0·86	13·6	16·30
C. { 50 per cent. nitro-cellulose (N = 12·24 per cent.) 50 per cent. nitro-glycerin }	21·15	60·67	2·67	15·58	41·0	23·1	0·08	—	0·4	15·58	20·01
D. { 50 per cent. nitro-cellulose (N = 13·30 per cent.) 50 per cent. nitro-glycerin }	20·47	61·23	2·49	16·35	45·3	19·0	0·00	—	0·3	16·35	19·90
E. { 80 per cent. nitro-cellulose (N = 12·24 per cent.) 20 per cent. nitro-glycerin }	24·37	58·98	2·98	14·0	28·9	38·4	0·05	—	1·0	14·0	18·2
F. { 80 per cent. nitro-cellulose (N = 13·30 per cent.) 20 per cent. nitro-glycerin }	23·11	58·98	2·71	15·84	33·4	32·6	0·04	—	0·7	15·84	18·2
G. { 35 per cent. nitro-cellulose (N = 13·30 per cent.) 5 per cent. vaseline ...... 60 per cent. nitro-glycerin }	22·2	59·0	2·88	15·46	33·0	31·3	0·2	—	0·7	15·46	19·0
H. Cordite, English manufacture	22·91	57·72	2·95	15·19	31·76	32·68	0·32	—	0·86	15·19	18·08
K. Ballistite, Italian and Spanish manufacture	21·47	60·83	2·68	15·80	41·11	23·76	0·12	—	0·47	15·8	19·69

and tri-nitro-cellulose in proportion corresponding to the nitrogen as found by analysis.

The equation for Experiment C may be taken as follows :—

$$\overbrace{6[C_3H_5(NO_3)_3]}^{\substack{\text{50 per cent.}\\ \text{nitro-glycerin.}}}+\overbrace{2[C_6H_7(NO_3)_3O_2]+3[C_6H_8(NO_3)_2O_3]}^{\substack{\text{50 per cent. nitro-cellulose}\\ (N = 12\cdot3 \text{ per cent.}).}} =$$
$$25CO_2+23CO+8H+30N+30H_2O.$$

The composition of this explosive, calculated from the foregoing formula and found by analysis, is as follows :—

	Formula.	Analysis.
C..................	21·2	21·15
O	60·8	60·67
H..................	2·5	2·67
N	15·5	15·58
	100·0	100·07

These are some of the principal features noticeable in a preliminary survey of these experiments. We are continuing our investigations on the lines indicated in the paper, and are especially endeavouring to measure the actual temperature of explosion under varying conditions, and it is hoped that the results obtained will throw some light on the chemical and physical properties of many gases at high temperatures and under considerable pressures, and, at the same time, be useful in the practical application of explosives.

IV. "On the Leicester Earthquake of August 4, 1893." By CHARLES DAVISON, M.A., Mathematical Master at King Edward's High School, Birmingham. Communicated by Professor J. H. POYNTING, F.R.S. Received February 28, 1894.

(Abstract.)

On August 4, 1893, at 6.41 P.M., an earthquake of intensity nearly equal to 6 (according to the Rossi-Forel scale) was felt over the whole of Leicestershire and Rutland and in parts of all the adjoining counties. The disturbed area was 58 miles long, 46 miles broad, and contained an area of about 2066 square miles. The direction of the longer axis (about W. 40° N. and E. 40° S.) and the relative position of the isoseismal lines show that the originating fault, if the earthquake were due to fault-slipping, must run in about the direction indicated, passing between Woodhouse Eaves and Markfield, and heading

towards the north-east. The anticlinal fault of Charnwood Forest, so far as known, satisfies these conditions, and it is highly probable that the earthquake was caused by a slip of this fault.

The beginning of the sound preceded that of the shock in all parts of the disturbed area; the end of the sound followed that of the shock in the central district and in the neighbourhood of the minor axis, but preceded it near the end of the major axis. Thus the sound apparently outraced the shock in the direction of the major axis, but not in that of the minor axis. These time-relations of the sound and shock can be readily explained if the area over which the fault-slip took place were several miles in length, for the sound in all probability is due to small and rapid vibrations proceeding chiefly from the margins of that area.

The intensity was greatest at and near Woodhouse Eaves, and it is probable that the fault-slip began in the neighbourhood of this place, gradually diminishing in amount in either direction, rather rapidly towards the north-west, and much more slowly towards the south-east; the rate at which the slipping advanced being greater than the velocity of the earth-wave. The total length of the fault-slip may have been as much as 12 miles or even more, and there can be little doubt that it was continued for some distance under the Triassic rocks on which Leicester is built.

V. "The Total Solar Eclipse of 16th April, 1893. Report on Results obtained with the Slit Spectroscopes." By E. H. Hills, Capt. R.E. Communicated by the Joint Solar Eclipse Committee. Received March 7, 1894.

The parties in Brazil and Africa were both supplied with these instruments, two being sent to each station. The instruments were arranged to take one photograph only during the eclipse with an exposure as long as possible. It was considered that the amount of light available would not allow of more than one successful exposure being made. Of the four resulting photographs, one of those taken in Brazil was unfortunately not finished before the sun reappeared, whilst the other shows a faint corona spectrum with a strong sky spectrum on both sides, and a considerable amount of general fog over the plate.

I have been able to detect nothing of interest in this photograph, for the Fraunhofer lines overlap the corona spectrum to such a degree that it is impossible to distinguish any bright lines with certainty.

The instrument employed in Africa consisted of two spectroscopes, on one equatorial mounting. The first spectroscope had two prisms,

each 1·75 in. height and 2·5 in. in base, with refracting angles of 62°, and the second spectroscope had one prism 2·6 in. both in height and base.

Condensing lenses, 3·5 in. aperture and 17·5 in. focus, and of 3 in. aperture and 14·5 in. focus, were used with the two instruments respectively.

Both spectroscopes were fixed on stout mahogany base-boards, and were completely adjusted before leaving England.

To attach them to the mounting, a mahogany tube, about 6 in. square and 2 ft. long, was bolted to the top of the declination axis, and the base-boards of the spectroscopes were screwed on either side of it.

A small telescope of 2½ in. aperture was attached on the other side of the tube to act as a finder and for purposes of adjustment. The mounting was one that was made for the eclipse of 1886. It consisted of a tripod stand composed of pieces of angle iron with the polar and declination axes, and circles of the Corbett equatorial. It was found to be easy to set up and rigid.

On arrival at Fundium, a site was selected, and a concrete base was formed. On this the instrument was set up, and no trouble was experienced in getting it into adjustment. The slits of the two spectroscopes were placed parallel to each other and tangential to a circle of declination, and were adjusted so that they cut across opposite limbs of the sun, that of the two-prism spectroscope being across the upper or western limb, and that of the one-prism spectroscope across the eastern limb. For several days before the eclipse, trial plates were taken, in order to obtain reference spectra, and for getting the focus as perfect as possible, as well as for the sake of practising the development of the plates.

The plates used were Cadett's most rapid make, and various developers were tried, but no special peculiarities of behaviour were noticed; pyrogallic acid was used for the eclipse plates. Before leaving England the plates were backed with a solution of asphalt in benzole, for the purpose of destroying the halation or reflection from the back surface of the glass.

At the eclipse the shutters of the two cameras were opened about ten seconds after the commencement of totality, and closed about ten seconds before the end, giving a total exposure of three minutes fifty seconds. During the progress of the eclipse I observed the corona and the upper or western limb of the sun through the small telescope with a magnifying power of 40. The corona in this region showed very faint radial markings and several rosy-pink prominences were seen. The largest of these was one at the W.N.W. limb, which is the one of which a strong spectrum was obtained with the two-prism spectroscope. The plates were developed the same evening on

board the "Alecto." The resulting photograph in the case of the two-prism spectroscope shows a prominence spectrum on both sides of the dark body of the moon, and outside these a corona spectrum with a faint solar (dark line) spectrum on its extreme edge. The H, K and some other lines extend over the dark moon and on both sides beyond the limits of the corona spectrum. That of the one-prism spectroscope shows the same general character, but there is a prominence spectrum on one side only. Both these photographs were over-exposed, better results would have been obtained if two or even three exposures had been made in the same time.

Measurement of the Photographs.

The following is the method of measurement adopted. A very accurate micrometer by Hilger, reading to 0·001 mm., was employed throughout.

The large number of bright lines in the prominence spectrum rendered the use of the reference spectra unnecessary.

The hydrogen series, together with the lines at wave-lengths 4215·3, 4471·2, and the *b* group gave a sufficient number of fixed points through which to draw an interpolation curve. The micrometer readings of these lines having been taken with the greatest possible accuracy, an interpolation curve was constructed on a large scale, two curves being drawn for each photograph as a check on each other. The micrometer readings of the remaining prominence lines were then determined and their wave-lengths taken from the curves.

The micrometer readings of the corona lines were next taken. It was impossible to get both sides of the photograph in the field of the microscope at the same time, so each side was taken separately, thus getting four series of scale-readings representing possible coronal lines.

The wave-lengths corresponding to these scale-readings were then determined from the interpolation curves, and lists were made—first, of lines common to both photographs; second, of lines occurring on both sides of one photograph; third, of lines which had been observed in previous eclipses.

New measurements of the photographs were again made, with the same care as the first, and all lines in the lists were struck out which were not plainly visible in this second scrutiny.

It is possible that this final list may contain some wave-lengths of lines due to accidental marks; this must be rare, however, as any mark so treated must have been parallel to the lines.

A comparison of the measurements of the two photographs will give a good idea of the limits of accuracy of these results.

The Prominence Spectrum.

This list gives the wave-lengths of all the lines in one prominence from each photograph. The second prominence on the two-prism spectroscope plate is of a similar character to the one given, but contains fewer lines.

The intensities of the lines are given approximately by the numbers from 1 to 6.

The most interesting feature of this spectrum is the extended hydrogen series. There seems no reason to doubt that the lines at wave-lengths 3692·5, 3687, 3682, 3678, 3675, 3672, 3669·5, and 3667 are members of it.

M. Deslandres has obtained a photograph showing five hydrogen lines beyond the one at wave-length 3699 ; this photograph carries the series three lines further. The line at 3680 is the iron line, whose wave-length is given by Cornu as 3680·3, and by Hartley as 3679·5. The new notation for the hydrogen series has been used as convenient. $H\beta$ is F, $H\gamma$ the line near G, and so on, consecutively.

The Corona Spectrum.

This is the final adopted list, as described above. It is almost impossible to estimate the intensity of these feeble lines by eye, so no attempt has been made to do so; but in the column headed "intensity" is placed the number of occurrences of the line in the two photographs, the maximum number being four, viz., on each side of both photographs.

Opposite each line in the table is placed the corresponding line that has been noted in previous eclipses. For a complete list of the observed corona spectrum, see Dr. Schuster's report on the eclipse of 1886 ('Phil. Trans.,' vol. 180 A, p. 335).

It will be observed that the 1474 K, or so-called corona line, is placed in the prominence and not in the corona spectrum. This line is shown very faintly on the extreme limit of one photograph, in which it certainly appears to belong to the prominence. It is true that it extends into the corona, but at the same time it also extends in the opposite direction, over the dark body of the moon. Its appearance is somewhat similar to that of the strong hydrogen lines, whose apparent extension into the corona spectrum is probably due to atmospheric haze.

This region of the spectrum has never been specially photographed with the slit spectroscope during an eclipse, and I think a serious attack on it should most certainly be made at the first opportunity, by using plates which can now be prepared, which are specially sensitive to this region of the spectrum.

Prominence Spectrum from Slit Spectroscope Photographs.

Intensity.	2-prism spectroscope.	1-prism spectroscope.	Reference.
1	3667·0		
1	3669·5		
1	3672·0		
1	3675·0		
1	3678·0		
4	3680·0	3680·0	
2	3682·0		
2	3687·0		
2	3692·5	3692·2	
3	3699·0	3699·0	Hξ
2	3700·0		
2	3701·0		
4	3707·5	3707·5	Hγ
3	3715·5	3715·1	
4	3716·9		
4	3718·0	3718·0	Hμ
1	3718·5		
1	3724·0		
1	3725·3		
4	3730·0	3730·0	Hλ
2	3732·8		
1	3737·3		
2	3741·3		
5	3745·5	3745·5	Hκ
3	3746·8		
5	3755·3	3755·0	
5	3757·4	3757·3	
1	3759·8		
1	3764·0		
5	3767·5	3767·5	Hι
5	3795·0	3795·0	Hθ
1	3813·5		
3	3817·7		
1	3822·5		
1	3823·6		
2	3827·5		
2	3828·5		
3	3830·8	3830·7	
5	3834·0	3834·0	Hη
5	3836·9	3836·5	
1	3839·6		
2	3855·8		
2	3858·8		
1	3866·5		
1	3877·1		
1	3880·5		
1	3882 8		
6	3888·0	3888·0	Hζ
1	3894·8		
1	3900·0		
1	3913·6		
6	3934·0	3934·0	K
1	3944·5		
1	3961·5		
6	3969·0	3969·0	Hε

Prominence Spectrum from Slit Spectroscope Photographs (*continued*).

Intensity.	2-prism spectroscope.	1-prism spectroscope.	Reference.
2	3986·9		
3	4026·6	4026·5	
1	4047·5		
3	4078·2	4078·4	Ca (Lockyer, 4078·2)
1	4092·5		
5	4101·2	4101·2	Hδ
3	4215·3	4215·3	Ca (Thalén, 4215·3)
1	4227·0	4226·5	Ca (Huggins, 4227; Thalén, 4226·3)
6	4340·0	4340·0	Hγ
4	4471·2	4471·2	*f*
6	4860·7	4860·7	Hβ
3	5015·0		
1	5169·1	..	b_3
1	5173·6	..	b_2
2	5184·2	..	b_1
1	5316·0	..	1474 K

Corona Spectrum from Slit Spectroscope Photographs.

Intensity.	2-prism spectroscope.	1-prism spectroscope.	Corresponding lines observed in previous eclipses.		
			1886.	1883.	1882.
2	3977·6	3977·0			
3	3982·6	3983·0			
2	3986·4	..	3986·0	3986	
2	3988·8				
1	3990·0	..	3990·0		
2	3992·5	3993·2	..	..	3992
2	3994·2	3995·0			
2	3998·8	3998·2	3998·4	3998	
3	4012·6	4011·9			
2	4015·6	4015·8	..	4016	4015
3	4022·0	4022·0			
2	4023·0	4023·8			
1	4031·6	..	4029·7	4031	
2	4039·3	4040·0	..	4037	
2	4054·0	4054·7	4054·8	4056	4057
2	4067·5	4067·8	4067·7	4064	4067
3	4070·5	4071·0	4071·0	4071	
2	4144·5	..	4144·2	4144	
2	4167·2	..	4166·0		
2	..	4169·0	4169·7	4169	4168
3	4175·0	4174·0	4173·6	..	4173
2	4181·2	4182·0	4183·5	4185	4179
4	4191·0	4190·3	4189·2	4192	4195
2	4202·1	4201·8			
2	..	4204·4	4203·5		

Corona Spectrum from Slit Spectroscope Photographs (*continued*).

Intensity.	2-prism spectroscope.	1-prism spectroscope.	Corresponding lines observed in previous eclipses.		
			1886.	1883.	1882.
2	4213·5	4212·2	4211·8	4213	4212
2	..	4224·4	4222·6	4227	4224
2	4267·5	4269·2	4268·5	..	4267
2	4279·7	4280·5	4280·6	4279	
2	4295·0	4295·3	4293·9	4291	
2	4299·5	4298·7	4301·0		
2	4328·3				
1	..	4331·5	4332·1	4330	
3	4353·0	4352·7	4354·7	4353	
3	4366·2	4364·9	4365·4	4363 (±3)	
4	4372·1	4372·4	4372·2	4370	4370
1	4378·5	..	4378·1	4377	
3	4386·7	4386·5	4387·6		
3	4389·5	4390·2	4389·0		
3	4395·2	4394·4	4395·8	..	4395
2	4447·5	..	4445·8	4449	
2	4454·7	4455·3	4452·9		
4	4465·4	4465·7	..	4465	
3	4468·8	4469·0	4468·5		
3	4494·0	4494·3	4493·4	4490	
3	4516·0	4516·3	4515·6	4518	
2	4530·2	4530·0	4530·0		
2	4536·0	..	4536·1		
2	4550·0	..	4550·0	4546	
1	4554·3	..	4557·2	4555 (±3)	
2	..	5020·0 (±2)			

VI. "The Stresses and Strains in Isotropic Elastic Solid Ellipsoids in Equilibrium under Bodily Forces derivable from a Potential of the Second Degree." By C. CHREE, M.A., Fellow of King's College, Cambridge, Superintendent of Kew Observatory. Communicated by Professor W. G. ADAMS, F.R.S. Received March 2, 1894.

(Abstract.)

If a system of bodily forces whose values per unit mass are derived from the potential

$$V = \tfrac{1}{2}(Px^2+Qy^2+Rz^2+2Syz+2Tzx+2Uxy)$$

acts on an ellipsoid

$$x^2/a^2+y^2/b^2+z^2/c^2 = 1,$$

whose density ρ is uniform, the statical resultant reduces to a couple whose components about the axes of x, y, z are respectively

$$4\pi abc\,(b^2-c^2)\,\rho S/15, \quad 4\pi abc\,(c^2-a^2)\,\rho T/15, \quad \text{and } 4\pi abc\,(a^2-b^2)\,\rho U/15.$$

These components vanish in the case of a sphere, but in an ordinary ellipsoid equilibrium will not exist unless S, T, U all vanish.

The problem solved in the present memoir, viz., that of an isotropic elastic solid ellipsoid under the action of bodily forces derived from a potential

$$\tfrac{1}{2}\,(Px^2+Qy^2+Rz^2),$$

is thus, for an ordinary ellipsoid, the most general case of equilibrium under forces derived from a potential of the second degree. The above potential covers forces arising from mutual gravitation or from rotation about a principal axis in an ellipsoid of any shape.

The method of solution reverses the usual order of procedure, the stresses being first determined and then the strains and displacements. The solution obtained satisfies without limitation or assumption of any kind all the elastic solid equations. Unless the ratios $a:b:c$ are assigned definite numerical values, the constant coefficients in the expressions for the stresses and strains are of course somewhat cumbrous; but for any specified case, whether of gravitation or rotation, or both combined, the solution becomes easily manageable. It enables the variation in the effects of gravitation and rotation with the change of shape of the ellipsoid to be completely traced.

The comprehensiveness of the problem solved forbids more than a brief consideration of the general solution with illustrations of its application to a few of the more interesting special forms of ellipsoid. The results obtained for the very oblate and very oblong forms seem to show that in many cases of bodily forces the assumptions usually made in the treatment of thin plates and long rods would not be justified.

By comparison with the author's previous researches, a close similarity is shown to exist between the phenomena in rotating flat ellipsoids and thin elliptic discs on the one hand, and rotating elongated ellipsoids and long elliptic cylinders on the other.

Various results confirmatory of the accuracy of the present solution are obtained by the application of the general formulæ for the mean strains in elastic solids. It is also shown that some of the results may be arrived at by the use of approximate but simple methods.

The Society adjourned over the Whitsuntide Recess to Thursday, May 24.

Presents, May 10, 1894.

Transactions.

Baltimore:—Johns Hopkins University. Studies in Historical and Political Science. Series 12. No. 3. 8vo. *Baltimore* 1894. The University.

Berlin:—Gesellschaft für Erdkunde. Verhandlungen. Bd. XXI. No. 4. 8vo. *Berlin* 1894; Zeitschrift. Bd. XXIX. No. 1. 8vo. *London* 1894. The Society.

Boston:—American Academy of Arts and Sciences. Proceedings. Vol. XXVIII. 8vo. *Boston* 1893. The Academy.

Chapel Hill:—Elisha Mitchell Scientific Society. Journal. Vol. X. Part 1. 8vo. *Raleigh, N.C.* 1893. The Society.

Cracow:—Académie des Sciences. Bulletin International. Mars, 1894. 8vo. *Cracovie.* The Academy.

Hertfordshire:—Hertfordshire Natural History Society and Field Club. Transactions. Vol. VII. Parts 8—9. 8vo. *London* 1894. The Club.

London:—British Astronomical Association. Journal. Vol. IV. No. 5. 8vo. *London* 1894; Memoirs. Vol. III. Part 1. 8vo. *London* 1894. The Association.

Geological Society. Quarterly Journal. Vol. L. Part 2. 8vo. *London* 1894. The Society.

Institute of Brewing. Transactions. Vol. VII. No. 6. 8vo. *London* 1894. The Institute.

Mathematical Society. Proceedings. Vol. XXV. Nos. 475—480. 8vo. *London* [1894]. The Society.

Photographic Society. Journal and Transactions. Vol. XVIII. No. 8. 8vo. *London* 1894. The Society.

Royal Medical and Chirurgical Society. Medico-Chirurgical Transactions. Vol. LXXVI. 8vo. *London* 1893; Catalogue of the Library. Supplement 7. 8vo. *London* 1893. The Society.

Royal Microscopical Society. Journal. 1894. Part 2. 8vo. *London* 1894. The Society.

Royal United Service Institution. Journal. Vol. XXXVIII. No. 194. 8vo. *London* 1894. The Institution.

Society of Biblical Archæology. Proceedings. Vol. XVI. Parts 5—6. 8vo. *London* 1894. The Society.

Zoological Society. Transactions. Vol. XIII. Part 8. 4to. *London* 1894; Proceedings. 1893. Part 4. 8vo. *London* 1894. The Society.

Lyons:—Université. Annales. Tome III. Fasc. 1—2. Tome VI. Fasc. 4. 8vo. *Paris* 1892–94. The University.

Transactions (*continued*).

Mexico :—Sociedad Científica "Antonio Alzate." Memorias y Revista. Tomo VII. Nos. 7—10. 8vo. *México* 1894.
The Society.

Naples :—Reale Accademia di Archeologia, Lettere e Belle Arti. Atti. Vol. XVI. 4to. *Napoli* 1894; Rendiconto. Anno VII—VIII. Gennaio e Febbraio. 8vo. *Napoli* 1893–94.
The Academy.

Newcastle-upon-Tyne :—North of England Institute of Mining and Mechanical Engineers. Transactions. Vol. XLIII. Part 4. 8vo. *Newcastle-upon-Tyne* 1893; An Account of the Strata of Northumberland and Durham as proved by Borings and Sinkings. L-R, S-T. 8vo. *Newcastle-upon-Tyne* 1887–94.
The Institute.

New York :—American Museum of Natural History. Bulletin. Vol. VI. Pp. 81—96. 8vo. [*New York*] 1894.
The Museum.

Philadelphia :—Geographical Club. Bulletin. Vol. I. No. 2. 8vo. *Philadelphia* 1894; Charter, By-Laws, List of Members. 8vo. *Philadelphia* 1894. The Club.

Santiago :—Sociedad Nacional de Mineria. Boletin. Año XI. No. 64. 4to. *Santiago de Chile* 1894. The Society.

Stockholm :—Kongl. Vetenskaps Akademie. Öfversigt. Årg. LI. Nos. 2—3. 8vo. *Stockholm* 1894. The Academy.

Toulouse :—Faculté des Sciences. Annales. Tome VIII. Fasc. 1. 4to. *Paris* 1894. The Faculty.

Utrecht :—Physiologisch Laboratorium der Hoogeschool. Onderzoekingen. Deel III. Afl. 1. 8vo. *Utrecht* 1894.
The Laboratory.

Vienna :—Kais. Akademie der Wissenschaften. Sitzungsberichte (Math.-Naturw. Classe). Bd. CII. Abth. 3. Heft 8—10. 8vo. *Wien* 1893; Sitzungsberichte (Phil.-Hist. Classe). Bd. CXXIX. 8vo. *Wien* 1893; Denkschriften. Bd. XLII. 4to. *Wien* 1893; Almanach. 1893. 8vo. *Wien;* Mittheilungen der Prähistorischen Commission. Bd. I. No. 3. 4to. *Wien* 1893. The Academy.

K.K. Geologische Reichsanstalt. Abhandlungen. Bd. VI. Hälfte 2. Bd. XV. Heft 6. 4to. *Wien* 1893; Jahrbuch. Bd. XLIII. Heft 3—4. 8vo. *Wien* 1894; Verhandlungen. 1894. Nos. 1—4. 8vo. *Wien.* The Institute.

Zurich :—Naturforschende Gesellschaft. Vierteljahrsschrift. Jahrg. XXXIX. Heft 1. 8vo. *Zürich* 1894.
The Society.

Observations and Reports.

Calcutta :—Meteorological Department, Government of India. Meteorological Observations recorded at Seven Stations in India. November, 1893. 4to. Monthly Weather Review. November, 1893. 4to. *Calcutta* 1894. The Department.

Edinburgh :—Royal Observatory. Circular. No. 42. 4to. [Sheet.] 1894. The Observatory.

India :—Archæological Survey Circle, North-Western Provinces and Oudh. Annual Progress Report. 1893. Folio. *Roorkee.* The Survey.

Geological Survey. Palæontologia Indica. Series IX. Vol. II. Part 1. Folio. *Calcutta* 1893; Records. Vol. XXVII. Part 1. 8vo. *Calcutta* 1894; a Manual of the Geology of India. Second edition. 8vo. *Calcutta* 1893. The Survey.

London :—Meteorological Office. Daily Weather Reports. July—December, 1892. January—June, 1893. 4to. [*London* 1892–93]; Meteorological Observations at Stations of the Second Order. 1889. 4to. *London* 1893. The Office.

Sydney :—Department of Agriculture. Plant Diseases and their Remedies, by N. A. Cobb. Diseases of the Sugar-Cane. 8vo. *Sydney* 1893. Dr. Cobb.

Washington :—U.S. Coast and Geodetic Survey. Report of the Superintendent. 1891. Part 2. 8vo. *Washington* 1892. The Survey.

U.S. Department of Agriculture. Report of the Ohio Weather and Crop Service. February, 1894. 8vo. *Norwalk, Ohio* 1894. The Department.

Journals.

Agricultural Gazette of New South Wales. Vol. V. Part 3. 8vo. *Sydney* 1894. Department of Agriculture, Sydney.

Boletin de Minas Industria y Construcciones. Año X. No. 2. 4to. *Lima* 1894. Escuela Especial de Ingenieros, Lima.

Horological Journal. Vol. XXXVI. No. 429. 8vo. *London* 1894. British Horological Institute.

Morphologisches Jahrbuch. Bd. XXI. Heft 2. 8vo. *Leipzig* 1894. Prof. Gegenbaur, For Mem. R.S.

Nature Notes. Vol. V. No. 53. 8vo. *London* 1894. Selborne Society.

Revue Médico-pharmaceutique. Année VII. No. 3. 4to. *Constantinople* 1894. The Editor.

Chauveau (A.), For. Mem. R.S. La Vie et l'Énergie chez l'Animal. 8vo. *Paris* 1894. The Author.

Clark (A. P.) [Nine Excerpts. 8vo. 1887–93.] The Author.

Dane (L. W.) Customary Law of the Main Tribes in the Gurdaspur District. 8vo. *Lahore* 1893. India Office.

Ferree (B.) The Chronology of the Cathedral Churches of France. 8vo. *New York* 1894. The Author.

[Hermite (C.), For. Mem. R.S.] Jubilé de M. Hermite. 1822—1892 (24 Décembre). 8vo. *Paris* 1893. Comité Hermite.

Hooker (Sir J. D.), F.R.S. The Flora of British India. Part 20. 8vo. *London* 1894. India Office.

Macfarlane (A.) The Principles of Elliptic and Hyperbolic Analysis. 8vo. *Boston, Mass.* 1894. The Author.

Maumené (E. J.) Extrait d'une Étude de l'Acide tétrafhique (tartrique) et de ses Composés Salins. 8vo. *Chateauroux* 1894. The Author.

Riccò (A.) La Lava Incandescente nel Cratere Centrale dell' Etna e Fenomeni Geodinamici concomitanti. Folio. *Roma* 1894; [and Six other Excerpts. 8vo. and 4to.] The Author.

Vallin (A. F.) Discursos leídos ante la Real Academia de Ciencias Exactas, Físicas y Naturales en la Recepción Pública de A. F. Vallin. 8vo. *Madrid* 1893. Sr. D. Vallin.

Wolf (R.) Astronomische Mitteilungen. No. 83. 8vo. [*Zürich*] 1894. The Author.

Bronze Copy of the Medal struck in honour of the 70th Birthday of M. Charles Hermite, For. Mem. R.S. Comité Hermite.

May 24, 1894.

The LORD KELVIN, D.C.L., LL.D., President, in the Chair.

Professor Éleuthère Élie Nicolas Mascart, who was elected a Foreign Member in 1882, signed the Obligation in the Charter Book, and was admitted into the Society.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Researches on the Electrical Properties of Pure Substances. No. I. The Electrical Properties of Pure Sulphur." By *RICHARD THRELFALL, M.A., Professor of Physics in the University of Sydney; JOSEPH HENRY DRAPIER BREARLEY, Deas-Thomson Scholar in the University of Sydney, and J. B. ALLEN, Exhibition Commissioners' Scholar of the University of Adelaide, South Australia. Communicated by Professor J. J. THOMSON, F.R.S. Received April 19, 1894.

(Abstract.)

Since there appears to be no definite information as to the electrical properties of pure elemental substances which are not metals, an attempt has been made to provide the necessary data in the case of sulphur. This element was chosen as being capable of easy purification, and because it can exist in a variety of forms, from the comparison of the electrical behaviour of which some information was expected to be obtained. The experimental work was begun in 1886, and some preliminary results were published by one of the authors and Mr. J. A. Pollock in the 'Philosophical Magazine' for 1890. These results referred to the construction of galvanometers for high resistance measurements, the reliability of the Clark cell as a source of small constant currents, and a method of using the galvanometer in resistance measurements in such a way that no galvanometer law of current measurement needs to be assumed.

* Part I by Professor Threlfall and Mr. Brearley. Part II by Professor Threlfall and Mr. Allen.

This method consists in observing the galvanometer indication of a current passing through the substance of high resistance under a known voltage, and subsequently causing the galvanometer to give the same deflection, by supplying it with a known fraction of the voltage of a Clark cell, and allowing this to act on the galvanometer when the latter is in series with a wire megohm standard. The discussion of this method, depending as it does on the behaviour of Clark cells from which currents are being taken, shows that it is reliable; but it is not intended here to go over the preliminary ground covered by the papers referred to. A considerable portion of the investigation of which the paper is an account, and which extends from 1889 to the present time (October, 1893), was made conjointly with Mr. Pollock.

The first part of the paper deals with the purification of sulphur as obtained from several sources, with the result that, in the end, the following method was exclusively adopted. This method is based on the use of sulphur recovered by the Chance process, which comes into commerce as pure to at least one part in ten thousand, and results from burning hydrogen sulphide from alkali waste with insufficient air for complete combustion. The commercial product is melted, and filtered through glass wool and platinum gauze. It is then twice distilled, in such a manner as to be free from exposure to dust: and sometimes it was subsequently freed from gas, by heating in a vacuum to near the boiling point. The purity of the resulting sulphur is tested by the following criteria. It must be free from smell. It must leave no residue on evaporation from a platinum dish. When cooled suddenly from a high temperature, it must remain of a clear yellow colour: when perfectly crystalline it must be absolutely soluble in carbon bisulphide. The absence of arsenic and selenium from the sulphur employed, was proved to about one part in a million by burning the sulphur to trioxide and applying the appropriate tests. The reaction by sulphur dioxide test for selenium, when properly carried out, is more delicate than the Marsh's test for arsenic, even when the smell of the hydrogen is adopted as a criterion. If a perceptible mirror of arsenic is to be accepted as a criterion, the arsenic test is still less delicate.

A number of experiments are described, tending to show that neither arsenic nor selenium can possibly exist to any appreciable extent in alkali waste produced in the Leblanc process, so that the Chance sulphur is probably more free from these impurities than the limit we can reach by analysis. All other known impurities are got rid of by distillation and exhaustion *in vacuo.*

Section 3 of the paper deals with a discussion of various methods of measuring high resistances, and gives the detail of the method adopted by us for rapidly effecting alternate measurements of

resistance and capacity. The method of producing films of pure sulphur between aluminium electrodes is also explained. It is necessary to perform the melting, &c., in a gold vessel.

Section 4 of the paper deals with the method of constructing galvanometers of high sensitiveness and resistance. In order to observe as small currents as possible, advantage was taken of every circumstance, both of observation and construction, likely to lead to enhanced sensitiveness. For instance, instead of observing the steady deflection, we habitually observed the throw of the needle on reversal of the current through the galvanometer. The steady deflection was only observed as a check.

The highest degree of sensitiveness we ever found it necessary to use, was such that the throw on reversal was 1 micrometer division for a current of $1 \cdot 5 \times 13^{-13}$ ampères, with a period of vibration of about 25 seconds. 1 micrometer division is divisible into five parts, so that the sensitiveness for least observable throw on reversal is 3×10^{-14} ampères. This sensitiveness, however, cannot be taken advantage of, except with very elaborate contact keys, and under rare conditions of magnetic steadiness. We adopted the Kelvin type of instrument. We consider that the problem of sensitive galvanometer building has not hitherto been approached in the proper manner. Almost any first-rate instrument will give enormous sensitiveness on occasion: but this sensitiveness is, in general, accompanied by instability, and is useless in practice, on account of zero changes. The really important matter is to ensure the presence of high sensitiveness with ease and certainty, not after hours of adjusting, but immediately on the necessity arising; in this we have been perfectly successful. Success in this matter depends entirely on a large number of details, for a discussion of which the paper must be consulted. Exact drawings are also provided, both of the instrument as a whole, and of the more important subsidiary parts. The following notes must suffice here.

1. It is essential that the coils shall be adjustable to the magnetic system after the latter is mounted.

2. Astaticism of sufficient perfection can only be secured by the simultaneous magnetisation of all the members of the magnetic system when they occupy their final relative positions. This necessitates special appliances.

3. If copper wire is used for the coils, no other metal must be included in the circuit or connections of the instrument, otherwise thermo-electric effects cannot be avoided.

4. The instrument has four tiers of coils and magnets, whereby improved electromagnetic conditions are obtained.

5. The most important part of the instrument is that which belongs to the adjusting of the magnetic control. This must be

exceedingly stiff and well made, supported quite independently of the astatic system, and capable of the finest adjustment.

6. Stability of zero depends chiefly on the uniformity of the controlling field all over the suspended system; this is, perhaps, best obtained by using very large and symmetrically disposed magnets above and below the suspended system.

7. When this is attended to, there is no advantage in using a "tail" magnet.

8. The chief remaining difficulty is found to be due to continual small changes taking place in the direction of the earth's horizontal field. This is best overcome by attending to the astaticism of the magnets, and using a fairly strong uniform controlling field opposed to that of the earth.

9. It is essential that the instrument be entirely surrounded by massive iron screens.

10. A novel method of illumination has been worked out, ensuring uniformity of brightness of the scale images, without any appreciable heating of the galvanometer. The transparent divisions of an opaque scale are caused to give rise to interference fringes, which are then observed in a telescope with a micrometer scale in the eyepiece. Readings of the position of the magnetic system to one second of arc can be easily and certainly made.

11. The most important improvements we have made relate to the insulation of the instrument, the minute adjustment of the controlling field, the recognition of the necessary conditions for high sensitiveness combined with stability, and the method of optical magnification. The instrument can now be used at the sensitiveness mentioned with all the ease and certainty which is generally attained with a millionfold less sensitiveness.

12. Further improvements can be made by using some material of greater strength than glass for the mirror, and by improved magnetic screening. Our screens were of cast iron, and weighed about 300 pounds; the screening was not nearly sufficiently perfect.

Section 5 contains an account of a large number of experiments extending over three years on the phenomena of conduction in sulphur—of which the following are the chief results.

Crystalline sulphur, whether monoclinic or "aged" monoclinic—(which we have ventured to distinguish as a distinct variety, since it preserves the melting point, but is divested of the crystallographic properties of fresh monoclinic sulphur) has a specific resistance of 10^{28} C.G.S. units as a minimum. By exposure to the air of a room the sulphur condenses moisture, which reduces its apparent specific resistance, but not nearly so much as in the parallel case of glass. The total residual charge is either absent, or less than four parts in ten thousand of the original charge, when a film of sulphur about a

quarter of a millimeter thick is charged for ten minutes with about 300 volts. By very careful drying we have succeeded in reducing the residual charge with a film of mica 0·2 mm. thick to about 1 per cent. of the original charge under similar circumstances.

In view of the want of homogeneity in the crystalline sulphur film this freedom from residual effect is noteworthy, and is perhaps to be explained by the entire absence of conductivity.

Crystalline sulphur has an electric strength which is more than enough to support 33,000 volts per centimeter—how much more we do not know. At 75° C the specific resistance with 285 volts per quarter millimeter falls to about $6{\cdot}8 \times 10^{25}$ C.G.S. The specific inductive capacity increases slightly as the temperature rises. As the temperature of the sulphur rises the conductivity increases slightly up to the melting point, when there is an enormous increase.

When a film containing about 5 per cent. of insoluble sulphur produced by cooling rapidly from a temperature above 170° C is examined, it is found to have a sensible conductivity which is not due to surface action, for it is not altered by fusing quartz rods into the exposed part of the surface, nor by blowing air saturated with water vapour against the surface. The conductivity depends on the exact composition of the mixture of soluble and insoluble sulphur, but may be taken at from 10^{25} to 10^{26} C.G.S. units for a film containing from three to six per cent. of amorphous unstable sulphur at ordinary temperatures. This conductivity is always greater when the voltage of about 300 volts on a film a quarter of a millimeter thick is first applied, or reversed. It is established that the increased conductivity occurs after the sulphur has rested—whether the voltage is applied for the first time, or whether it has been applied before in either direction. When the voltage is reversed this effect is more strongly marked, and the conductivity only settles to a steady value after a considerable time. The conduction, either when the current is steady, or when it commences or is reversed, does not obey Ohm's law either for small voltages (say eight volts) or large ones (say 300) when the film is 0·25 mm. thick. The deviation is, however, greater at high voltages, and greater when the "commencing" or "reversing" effects are taking place than when the conduction is steady. The deviation is always in the direction of making the conduction at high electromotive intensities greater than at low. The specific inductive capacity of a mixture of soluble and insoluble sulphur is markedly higher than that of purely crystalline sulphur. We have some evidence that the changes occurring during the first few days after the film is made lead to an increase of specific inductive capacity. The temperature coefficient of the specific inductive capacity is positive, and of the order 2×10^{-6} per degree between 20° and 70° C.

The residual charge is larger than when the sulphur is purely soluble, and with about 400 volts per millimeter is about 187 per cent. of the initial charge after 10 minutes' charge; the condenser being discharged for a fraction of a second and left for 10 minutes. At lower electromotive intensities it is rather greater in comparison with the initial charge, sufficiently so to be distinctly noticeable.

On heating the sulphur the conduction increases from about 50° C, in fact whenever the process of annealing takes place. When the annealing change (destruction of amorphous sulphur, and formation of soluble sulphur) is taking place rapidly the conduction is considerable. Many attempts made for the purpose of deciding whether the increase of conductivity depends on the mere proportion of insoluble sulphur present, or whether it depends on the rate at which the conversion process is taking place, yielded no absolutely certain results, but the evidence, such as it is, points to the latter as being probably the most important—but we do not consider that it can explain the conductivity at low temperatures. This conductivity is essentially discontinuous, and in this resembles the conduction through moisture films condensed on glass, ebonite, and sulphur itself.

Several of the above-mentioned peculiarities of sulphur conduction were observed by Quincke in the case of insulating liquids, and were ascribed, in part at all events, to the action of dust motes in the liquids. There is no doubt, however, that in the case of sulphur these effects are inherent to the process of conduction, for they were as strongly marked in what we consider to have been our purest film (as tested by the colour) as in the least pure one. There is some evidence that mixtures of insoluble and soluble sulphur show a maximum conductivity when the sulphur contact is between 5 and 3 per cent. All the phenomena of conduction are also noticed—the specific resistance being about the same—when we examine films containing 88 per cent. of insoluble sulphur, produced by applying enormous pressures in a testing machine to the insoluble sulphur formed on suddenly cooling sulphur from a high temperature. The sulphur, which was originally plastic, was exhausted with carbon bisulphide, and the residue treated with sulphur chloride to obtain stability. A pressure equal to the weight of 100,000 pounds on an area of say 25 square inches, causes about 12 per cent. of the insoluble sulphur to become soluble, whether it has been treated with sulphur chloride or not. Check experiments on soluble sulphur showed that the very pure benzene used to moisten the sulphur for the purpose of compression produces no subsequent change in the conductivity. The pressures were applied for from five to 10 minutes.

No change in the conductivity of mixed films was produced by stressing in alternate directions with a frequency of, say, five per second, and a voltage of from 100 to 200 volts per quarter millimeter.

A very large quantity of exact numerical data referring to these points is contained in the paper.

Part II of the paper is interposed as the facts disclosed bear on the general argument. This part of the paper bears on several correlated questions.

Section 1 deals with the contact force in air between purely soluble sulphur and mixtures of insoluble and soluble sulphur containing about 10 per cent. of the former.

The result of some rather interesting work on this point by the electrometer needle method, shows that when soluble and mixed sulphur is in contact (produced by melting the parts together), there is a contact force of the order of from one to two volts between them. The positive charge is on the insoluble sulphur. These experiments were made by using a double sulphur needle over metallic semicircles, and also by using the ordinary metallic electrometer needle over sulphur quadrants. The latter gives the best results. The phenomena are very complicated, and require to be carefully sifted; for an account of the very considerable difficulties the paper must be consulted.

Section 2 deals with the question as to whether light has any effect on the conductivity of sulphur. Monckman ('Roy. Soc. Proc.,' vol. 46, 1890) considers that he has discovered such an effect. A very large number of experiments, however, on mixed and crystalline sulphur cells, failed to indicate to us any such peculiarity, and we consider that Monckman must have been mistaken in this matter.

Section 3 deals with the qualitative phenomena of conduction in sulphur cells containing from 5 per cent. to 20 per cent. of insoluble sulphur. The general results agree with those already described, although the methods of preparing and quenching the viscous sulphur were different. The electrodes were also of platinum wire instead of aluminium plates. The temperature resistance changes are treated rather fully in this section, and bring into prominence the enormous influence of the annealing process.

Section 4 deals with a determination of the specific inductive capacity of sulphur by the method of weighing, and contains an account of the different sources of error to which we discovered the method was subject. Several ways of obtaining the required potential difference were investigated, with the result that the most satisfactory is by the use of an alternator giving a frequency of about sixty, and an induction coil used as a transformer. This avoids the difficulty which occurs when the sulphur plates get charged in virtue of their conductivity, and is noticed whenever (1) a continuously directed P.D., or (2) an unequal alternating one (as by a coil with hammer or mercury break) is used.

Part I is then continued. § 6 deals with an investigation of the specific inductive capacity of various kinds of sulphur by the method of weighing, advantage being taken of the laborious investigation of the method dealt with in Part II. Various other matters came to light, and we furnish a drawing of suitable apparatus and describe the necessary course of procedure to make the method accurate and satisfactory; in particular the proper way of preparing plates of crystalline and friable substances. The results for the specific inductive capacities are as follows, at a temperature of 14° C.

"Aged" monoclinic sulphur	$K = 3{\cdot}162$
Ditto with 1·43 per cent. insoluble unstable sulphur	$K = 3{\cdot}510$
Ditto with 3 per cent. insoluble unstable sulphur	$K = 3{\cdot}75$

An experiment on purely amorphous sulphur is not yet ready for publication; but the above results will go a long way to clear up the great differences in the hitherto published values of this constant for sulphur. They also serve as a check on our observations on thin films, and show that our measurements of film thickness—a grave difficulty—were moderately successful.

A number of experiments bearing on a theory of conduction which we venture to suggest are also included in this part of the paper.

This is followed by an account of the theory to which our experiments led us, and which is briefly as follows. Sulphur in either of the extreme conditions does not conduct; we can only examine the purely soluble state, for the other is not sufficiently stable for a satisfactory investigation; however, we may say that changing the content of amorphous unstable sulphur from 3 per cent. to 88 per cent., produces little or no change in the conductivity. Taking this and other facts into consideration, we believe that what we have called mixtures of the two kinds of sulphur are really compounds, and that the conduction is electrolytic.

We have framed what we believe to be a novel theory of electrolysis, which explains all the facts which we have observed, and which has the peculiarity of introducing the idea of an electrolytic convection current, in connection with which the resulting changes of specific inductive capacity allow of all the phenomena of conduction observed taking place, though the charges may never really reach the electrodes. It will be seen that the effects of fatigue-reversal and the phenomena of discontinuous conduction are well accounted for by this theory. The only objections we have to it are that it is based on a molecular theory of matter, which we are persuaded requires to be remodelled, if it is to afford any real explanation of things as they are. A theory of residual effect based on the theory of conduction is

also proposed. This differs from Maxwell's theory in that the latter merely postulates changes of specific resistance and specific inductive capacity from point to point of the dielectric, while our theory is distinctly chemical. We consider that our results on mixed films are best explained by the theory we propose, though the difficulty of disproving Maxwell's theory is almost equal to the difficulty of establishing it, and we do not wish to imply that some sort of explanation on this theory may not be constructed to fit in with our observations. This is a point, however, on which we are still engaged. The matter may, perhaps, be best summed up in the statement that the evidence we have against Maxwell's theory is nearly worthless; but that we do not consider this theory necessary if our theory of conduction be accepted.

II. "On the Dynamical Theory of Incompressible Viscous Fluids and the Determination of the Criterion." By Osborne Reynolds, F.R.S., &c. Received April 25, 1894.

(Abstract.)

The equations of motion of viscous fluid (obtained by grafting on certain terms to the abstract equations of the Eulerian form so as to adapt these equations to the case of fluids subject to stresses depending in some hypothetical manner on the rates of distortion, which equations Navier* seems to have first introduced in 1822, and which were much studied by Cauchy† and Poisson‡) were finally shown by St. Venant§ and Sir Gabriel Stokes,‖ in 1845, to involve no other assumption than that the stresses, other than that of pressure uniform in all directions, are linear functions of the rates of distortion with a co-efficient depending on the physical state of the fluid.

By obtaining a singular solution of these equations as applied to the case of pendulums in steady periodic motion Sir G. Stokes¶ was able to compare the theoretical results with the numerous experiments that had been recorded, with the result that the theoretical calculations agreed so closely with the experimental determinations as seemingly to prove the truth of the assumption involved. This was also the result of comparing the flow of water through uniform tubes with the flow calculated from a singular solution of the equations so long as the tubes were small and the velocities slow. On the other

* 'Mém. de l'Académie,' t. vi, p. 389.

† 'Mém. des Savants Etrangers,' t. 1, p. 40.

‡ 'Mém. de l'Académie,' t. x, p. 345.

§ 'B.A. Report,' 1846.

‖ 'Cambridge Trans.,' 1845.

¶ 'Cambridge Trans.,' vol. ix, 1857.

hand, these results, both theoretical and practical, were directly at variance with common experience as to the resistance encountered by larger bodies moving with higher velocities through water, or by water moving with greater velocities through larger tubes. This discrepancy Sir G. Stokes considered as probably resulting from eddies which rendered the actual motion other than that to which the singular solution referred and not as disproving the assumption.

In 1850, after Joule's discovery of the Mechanical Equivalent of Heat, Stokes showed, by transforming the equations of motion—with arbitrary stresses—so as to obtain the equation of ("Vis-viva") energy, that this equation contained a definite function, which represented the difference between the work done on the fluid by the stresses and the rate of increase of the energy per unit of volume, which function, he concluded, must, according to Joule, represent the Vis-viva converted into heat.

This conclusion was obtained from the equations irrespective of any particular relation between the stresses and the rates of distortion. Sir G. Stokes, however, translated the function into an expression in terms of the rates of distortion, which expression has since been named by Lord Rayleigh the *Dissipation Function.*

In 1883 the author succeeded in proving, by means of experiments with colour bands—the results of which were communicated to the Society*—that when water is caused by pressure to flow through a uniform smooth pipe, the motion of the water is *direct*, *i.e.*, parallel to the sides of the pipe, or *sinuous*, *i.e.*, crossing and recrossing the pipe, according as U_m, the mean velocity of the water, as measured by dividing Q, the discharge by Δ, the area of the section of the pipe, is below or above a certain value given by $K\mu/D\rho$, where D is the diameter of the pipe, ρ the density of the water, and K a numerical constant, the value of which according to the author's experiments and, as he was able to show, to all the experiments by Poiseuille and Darcy, is for pipes of circular section between

$$1{,}900 \text{ and } 2{,}000,$$

or, in other words, steady direct motion in round tubes is stable or unstable according as

$$\rho\frac{DU_m}{\mu} < 1900 \quad \text{or} \quad > 2000$$

the number K being thus a criterion of the possible maintenance of sinuous or eddying motion.

The experiments also showed that K was equally a criterion of the law of the resistance to be overcome—which changes from a

* 'Phil. Trans.,' 1883, Part III, p. 935.

resistance proportional to the velocity and in exact accordance with the theoretical results obtained from the singular solution of the equation, when direct motion changes to sinuous, *i.e.*, when

$$\rho \frac{DU_m}{\mu} = K.$$

In the same paper it was pointed out that the existence of this sudden change in the law of motion of fluids between solid surfaces when

$$DU_m = \frac{\mu}{\rho} K,$$

proved the dependence of the manner of motion of the fluid on a relation between the product of the dimensions of the pipe multiplied by the velocity of the fluid and the product of the molecular dimensions multiplied by the molecular velocities which determine the value of μ for the fluid, also that the equations of motion for viscous fluid contained evidence of this relation.

These experimental results completely removed the discrepancy previously noticed, showing that, whatever may be the cause, in those cases in which the experimental results do not accord with those obtained by the singular solution of the equations, the actual motions of the water are different. But in this there is only a partial explanation, for there remains the mechanical or physical significance of the existence of the criterion to be explained.

In the present paper the author applies the dynamical theory to the motion of incompressible viscous fluids to show—

(*a*.) That the adoption of the conclusion arrived at by Sir Gabriel Stokes, that the dissipation function represents the rate at which heat is produced, adds a definition to the meaning of u, v, w—the components of mean or fluid velocity—which was previously wanting;

(*b*.) That as the result of this definition the equations are true, and are only true, as applied to fluid in which the mean-motions of the matter, excluding the heat motions, are steady;

(*c*.) That the evidence of the possible existence of such steady mean-motions, while at the same time the conversion of the energy of these mean-motions into heat is going on, proves the existence of some *discriminative cause* by which the *periods* in space and time of the mean-motion are prevented from approximating in magnitude to the corresponding *periods* of the heat motions; and also proves the existence of some general action by which the energy of mean-motion is continually *transformed* into the energy of heat-motion without passing through any intermediate stage;

(*d*.) That as applied to fluid in unsteady mean-motion (excluding

the heat-motions), however steady the mean integral flow may be, the equations are approximately true in a degree which increases with the ratios of the magnitudes of the *periods*, in time and space, of the mean-motion to the magnitude of the corresponding periods of the heat-motions;

(*e.*) That if the *discriminative cause* and the *action of transformation* are the result of general properties of matter, and not of properties which affect only the ultimate motions, there must exist evidence of similar actions as between mean-mean-motion, in directions of mean flow, and the periodic mean-motions taken relative to the mean-mean-motion but excluding heat-motions. And that such evidence must be of a general and important kind, such as the unexplained laws of the resistance of fluid motions, the law of the universal dissipation of energy and the second law of thermodynamics;

(*f.*) That the *generality* of the effects of the properties on which the *action of transformation* depends is proved by the evidence that resistance, other than proportional to the velocity, is caused by the relative (eddying) mean-motion.

(*g.*) That the existence of the *discriminative cause* is directly proved by the existence of the *criterion*, the dependence of which on circumstances which limit the magnitudes of the periods of relative-mean-motion, as compared with the heat motion, also proves the *generality* of the effects of the properties on which it depends.

(*h.*) That the proof of the generality of the effects of the properties on which the discriminative cause and the action of transformation depend, shows that—if in the equations of motion the mean-mean-motion is distinguished from the relative-mean-motion in the same way as the mean-motion is distinguished from the heat-motions—(1) the equations must contain expressions for the *transformation* of the energy of mean-mean-motion to energy of relative-mean-motion; and (2) that the equation, when integrated over a complete system, must show that the possibility of relative-mean-motion depends on the ratio of the possible magnitudes of the periods of relative-mean-motion, as compared with the corresponding magnitude of the periods of the heat-motions.

(*i.*) That when the equations are transformed so as to distinguish between the mean-mean-motions of infinite periods and the relative-mean-motion of finite periods, there result two distinct systems of equations, one system for mean-mean-motion, as affected by relative-mean-motions and heat-motion, the other system for relative-mean-motion as affected by mean-mean-motion and heat-motions.

(*j.*) That the equation of energy of mean-mean-motion, as obtained from the first system, shows that the rate of increase of energy is diminished by conversion into heat, and by transformation of energy of mean-mean-motion in consequence of the relative-mean-

motion, which transformation is expressed by a function identical in form with that which expresses the conversion into heat; and that the equation of energy of relative-mean-motion, obtained from the second system, shows that this energy is increased only by transformation of energy from mean-mean-motion expressed by the same function, and diminished only by the conversion of energy of relative-mean-motion into heat.

(*k.*) That the difference of the two rates (1) transformation of energy of mean-mean-motion into energy of relative-mean-motion as expressed by the transformation function, (2) the conversion of energy of relative-mean-motion into heat, as expressed by the function expressing dissipation of the energy of relative-mean-motion, affords a discriminating equation as to the conditions under which relative-mean-motion can be maintained.

(*l.*) That this discriminating equation is independent of the energy of relative-mean-motion, and expresses a relation between variations of mean-mean-motion of the first order, the space periods of relative-mean-motion and μ/ρ such that any circumstances which determine the maximum periods of the relative-mean-motion determine the conditions of mean-mean-motion under which relative mean-motion will be maintained—determine *the criterion :*

(*m.*) That as applied to water in steady mean flow between parallel plane surfaces, the boundary conditions and the equation of continuity impose limits to the maximum space periods of relative-mean-motion such that the discriminating equation affords definite proof that when an indefinitely small sinuous or relative disturbance exists it must fade away if

$$\rho\frac{DU_m}{\mu}$$

is less than a certain number, which depends on the shape of the section of the boundaries and is constant as long as there is geometrical similarity. While for greater values of this function, in so far as the discriminating equation shows, the energy of sinuous motion may increase until it reaches to a definite limit, and rules the resistance.

(*n.*) That besides thus affording a mechanical explanation of the existence of the criterion K, the discriminating equation shows the purely geometrical circumstances on which the value of K depends, and although these circumstances must satisfy geometrical conditions required for steady mean-motion other than those imposed by the conservations of mean energy and momentum, the theory admits of the determination of an inferior limit to the value of K under any definite boundary conditions, which, as determined for the particular case, is

517.

This is below the experimental value for round pipes, and is about half what might be expected to be the experimental value for a flat pipe, which leaves a margin to meet the other kinematical conditions for steady mean-mean-motion.

(*o.*) That the discriminating equation also affords a definite expression for the resistance, which proves that, with smooth fixed boundaries, the conditions of dynamical similarity under any geometrical similar circumstances depend only on the value of

$$\frac{\rho}{\mu^2}\frac{dp}{dx}b^3,$$

where b is one of the lateral dimensions of the pipe; and that the expression for this resistance is complex, but shows that above the critical velocity the relative-mean-motion is limited, and that the resistances increase as a power of the velocity higher than the first.

III. "On certain Functions connected with Tesseral Harmonics, with Applications." By A. H. LEAHY, M.A., late Fellow of Pembroke College, Cambridge, Professor of Mathematics at Firth College, Sheffield. Communicated by Professor W. M. HICKS, F.R.S. Received March 24, 1894.

(Abstract.)

The transformation of a zonal harmonic referred to a pole on a sphere to another pole on the same sphere, and its expression in a series containing the $2n+1$ harmonics of the same order referred to this new pole, is an operation frequently employed in physical research. The purpose of this paper is the investigation of certain functions of the angular distance between the poles which occur when a general tesseral harmonic is transformed from one pole and plane to another pole and another plane of reference. If the coordinates of any point on the sphere when referred to the first pole are β' and γ'; β' denoting the colatitude, and γ' the longitude; and if the coordinates of the same point when referred to the second pole are δ' and q; δ' denoting the colatitude and q the longitude referred to a plane through the two poles, it is shown that

$$\mathrm{P}(n,m,\mu')\cos m\gamma' = \cos m\gamma\left\{u_{m,0}\,.\,\mathrm{P}_n(\nu)+2\Sigma\frac{n-r\,!}{n+r\,!}u_{mr}\,.\,\mathrm{P}(n,r,\nu)\cos rq\right\}$$
$$+\sin m\gamma\,.\,2\Sigma\frac{n-r\,!}{n+r\,!}v_{mr}\,.\,\mathrm{P}(n,r,\nu)\sin rq,$$

$$P(n,m,\mu')\sin m\gamma' = \sin m\gamma\left\{u_{m,o}.P_n(\nu) + 2\Sigma\frac{n-r\,!}{n+r\,!}u_{mr}.P(n,r,\nu)\cos rq\right\}$$

$$-\cos m\gamma\,.\,2\Sigma\frac{n-r\,!}{n+r\,!}v_{mr}.P(n,r,\nu)\sin rq,$$

where $P(n,m,\mu')$ is the "associated function" $\frac{d^mP_n(\mu')}{d\mu'^m}\,.\,(1-\mu'^2)^{m/2}$; μ' and ν are put for $\cos\beta'$, $\cos\delta'$, γ is the longitude of the second pole referred to the original pole and plane, and u_{mr} v_{mr} are the functions of β, the angular distance between the poles whose properties are discussed in the paper. When m is zero, *i.e.*, when $P(n,m,\mu')$ is a zonal harmonic, the function u_{mr} reduces to $P(n,r,\mu)$, if μ is put for $\cos\beta$, and v_{mr} is zero. The general equations connecting the functions, and the values of the functions for general and for particular values of m and r are investigated.

If δ' and ϵ' are the colatitude and longitude of a point referred to the second pole and any plane through the pole, the integral of the product of any two tesseral harmonics both of the nth order over the surface of a sphere can be expressed concisely in terms of the functions u_{mr}, v_{mr}. The result is

$$\iint P(n,m,\mu')\cos(m\gamma'+\alpha)\,.\,P(n,m,\nu)\cos(r\epsilon'+\rho)\,dS =$$

$$\frac{4\pi a^2}{2n+1}\{\cos(m\gamma+\alpha)\cos(r\epsilon+\rho)\,u_{mr}(\beta) - \sin(m\gamma+\alpha)\sin(r\epsilon+\rho)v_{mr}(\beta)\}$$

if γ is the longitude of the second pole referred to the plane through the first, ϵ the longitude of the first pole referred to the plane through the second, β is the angular distance between the poles, and α, ρ are constants.

The functions u_{mr}, v_{mr} are connected by several equations, bearing a great resemblance to equations connecting tesseral harmonics of the same order. They are of course functions of n, and should be written, when n may have different values, in the form $u_{n,m,r}, v_{n,m,r}$, but the n is omitted for brevity in most of the results. Some of the most important results are the following, the dashes denoting differential coefficients—

$$u''_{mr}\sin\beta + u'_{mr}\cos\beta + \{n(n+1)\sin\beta - (m^2+r^2)\operatorname{cosec}\beta\}u_{mr}$$
$$= 2mrv_{mr}\cot\beta \;\;\ldots\ldots\;\; (21);$$

$$u_{m,r+1} + 2u'_{mr} - (n+r)(n-r+1)\,u_{m,r-1} = 0 \;\;\ldots\ldots\;\; (24);$$

$$u_{m,r+1} - 2r\cot\beta\,u_{mr} + (n+r)(n-r+1)\,u_{m,r-1} = 2m\operatorname{cosec}\beta\,v_{mr}$$
$$\ldots\ldots\;\; (27).$$

All the relations connecting u_{mr}, v_{mr}, &c., are duplicate ones, similar relations being obtained by interchanging u and v.

The differential equation satisfied by either function is of the fourth order, the two functions being different solutions of this equation. The two remaining solutions of the equation have also been obtained, and called "functions of the second kind." The equation of finite differences satisfied by the functions is also of the fourth order.

The general value of u_{mr} is—

$$2u_{mr} = \mathrm{P}(n, m+r, \mu)$$

$$+ \sum_{k=1}^{k=\mathrm{I}(r/2)} (-1)^k . rm(m+r-2k)\, \mathrm{P}(n, m+r-2k, \mu)$$

$$\times \sum_{s=1}^{s=k} (-1)^s \frac{m-k+s-1\,!\; m+r-k-1\,!\; n+k-s\,!\; r-k+s-1\,!\; k-1\,!}{m-k\,!\; m+r-k-s\,!\; n-k+s\,!\; r-k\,!\; s-1\,!\; k-s\,!\; s\,!}$$

$$+ \sum_{k=\mathrm{I}(r/2)+1}^{k=r-1} (-1)^k rm(m+r-2k) \frac{n+m\,!\; n-m+2k-r\,!}{n-m\,!\; n+m-2k+r\,!} \mathrm{P}(n, m+r-2k, \mu)$$

$$\times \sum_{s=1}^{s=r-k} (-1)^s \frac{m-k+s-1\,!\, m+r-k-1\,!\, n+r-k-s\,!\, r-k+1\,!\, k+s-1\,!}{m-k\,!\, m+r-k-s\,!\, n-r+k+s\,!\, r-k-s\,!\, s\,!\, k\,!\, s-1\,!}$$

$$+(-1)^r . \frac{n+m\,!\, n-m+r\,!}{n-m\,!\, n+m-r\,!} \mathrm{P}(n, m-r, \mu),$$

where $\mathrm{I}(r/2)$ is the greatest integer in $r/2$.

The value of v_{mr} is given by

$$v_{mr} . \sin\beta = \sum_{k=0}^{k=\mathrm{I}\left(\frac{r-1}{2}\right)} (-1)^k (m+r-2k-1)\, \mathrm{P}(n, m+r-2k-1, \mu)$$

$$\times \sum_{s=0}^{s=k} (-1)^s \frac{m-k+s-1\,!\, m+r-k-1\,!\, n+k-s\,!\, r-k+s-1\,!\, k\,!}{m-k-1\,!\, m+r-k-s-1\,!\, n-k+s\,!\, r-k-1\,!\, s\,!\, k-s\,!\, s\,!}$$

$$+ \sum_{k=\mathrm{I}\left(\frac{r-1}{2}\right)+1}^{k=r-1} (-1)^k (m+r-2k-1) \frac{n+m\,!\; n-m+2k-r-1\,!}{n-m\,!\; n+m-2k+r+1\,!} \mathrm{P}(n, m+r-2k-1, \mu)$$

$$\sum_{s=0}^{s=r-k-1} (-1)^s \frac{m-k+s-1\,!\, m+r-k-1\,!\, n+r-k-s-1\,!\, r-k-1\,!\, k+s\,!}{m-k-1\,!\, m+r-k-s-1\,!\, n-r+k+s+1\,!\, r-k-s-1\,!\, s\,!\, k\,!\, s\,!}$$

Simpler values for u_{mr} v_{mr} are given for general values of m from $r=0$ to $r=6$ inclusive.

The values of the functions u_{mr}, v_{mr} are of a simpler form when β is

a right angle, and can be expressed by a single series. When $n-r$ is even, the series

$$(-1)^{\frac{n-r}{2}} \frac{\frac{n-r}{2}!\,\frac{n+r}{2}!\,m!\,m!}{n-r!\,2m!} 2^{-n}$$

$$\sum_{t=0} (-1)^t \frac{n+2m-r-2t!\,n+r-2t!}{m-t!\,t!\,\frac{n+r}{2}+t!\,\frac{n-r}{2}-t!\,\frac{n+r}{2}-m+t!\,\frac{n-r}{2}+m-t!}$$

is the value of $u_{mr}(\pi/2)$ when m is even, and of $v_{mr}(\pi/2)$ when m is odd; the series being continued until one of the factorials in the denominator becomes negative; and n being supposed greater than $2m$. When n is less than $2m$, the lower limit of t is $m-\frac{1}{2}(n+r)$.

A similar series gives the values of $u_{mr}(\pi/2)$ when m is odd, and of $v_{mr}(\pi/2)$ when m is even for the case when $n-r$ is odd. The values of $u_{mr}(\pi/2)$ when m is even and of $v_{mr}(\pi/2)$ when m is odd, are in this case equal to zero.

When $n-r$ is even, the values of $u_{mr}(\pi/2)$ when m is odd, and of $v_{mr}(\pi/2)$ when m is even, are also equal to zero.

The value of u_{mr} is in all cases equal to u_{rm}, and the value of v_{mr} is equal to v_{rm}. This result gives several algebraic identities, using general values of u_{mr}. Since $u_{o,r} = \mathrm{P}(n,r,\mu)$, we have by this result $u_{m,o} = \mathrm{P}(n,m,\mu)$, whence we get the result that

$$\int \mathrm{P}\,(n,m,\mu')\cos m\gamma' dq = 2\pi \mathrm{P}_n\,(\nu)\,.\,\mathrm{P}\,(n,m,\mu)\cos m\gamma.$$

Thus the line integral of a Laplace's function referred to the first pole along a small circle described about the second pole at angular distance δ from it is the value of the function at the second pole multiplied by $2\pi \mathrm{P}_n(\nu)\,.\sin\delta$, where ν is $\cos\delta$.

Equations can also be obtained connecting $u_{n,m,r}$ and $u_{n+1m,r}$, where the n's are different. The most useful result is

$$n\,(n-m+1)\,(n-r+1)\,u_{n+1,m,r}-(2n+1)\,n\,(n+1)\cos\beta u_{n,m,r}$$
$$+(n+1)\,(n+m)\,(n+r)\,u_{n-1,m,r} = (2n+1)\,mrv_{n,m,r} \;\ldots\ldots\; (45),$$

and a similar equation obtained by interchanging u and v. From this equation a table of the functions for different values of n can be calculated, and is given from $n=0$ to $n=4$. Since $v_{m,o} = v_{o,m} = 0$, the number of the functions for any given value of n is $(n+1)^2+n^2$.

Two physical applications of the results are given. The first is an application of the result of a line integral of a Laplace's function referred to one pole along a small circle described about another. The result is employed to establish that the law assumed by Boltzmann and ell for the number of particles which have a given velocity in

an irregular system of moving molecules (or a "disturbed gas") is unaltered in form by collisions between the molecules. In the second application the functions are used to find the mutual potential energy of two layers of gravitating matter on two spheres, the density at any point on each sphere being expressed in terms of spherical harmonics referred to fixed coordinates upon it, and the spheres having any position with reference to the line joining their centres. The case of two ellipsoids not differing much from spheres is also worked out numerically, and the stable positions discussed. A stable orbit is possible with the major axes of the ellipsoids constantly in a straight line. If one ellipsoid is fixed and the other projected so as to describe a nearly circular orbit about it, with its major axis initially pointing to the centre of the other, the orbit will be possible if in a plane perpendicular to the least axis of the greater, but the deviation of the major axis of the second from the line of centres will contain a term which to the first approximation is secular, and may ultimately cause this axis to deviate from its initial position. There are three stable positions for the second ellipsoid if the first ellipsoid is fixed and the centre of the other fixed. These positions will in general be with the major axis of the second pointing towards the centre of the first, and in a line with the major, mean, and least axes of the first, but if c, the distance between the centres, is so small that

$$5\left(\frac{2}{a_1^2}-\frac{1}{a_1'^2}-\frac{1}{a_1''^2}\right)c^2 < \left(\frac{12}{a_1^2}-\frac{7}{a_1''^2}-\frac{5}{a_1'^2}\right)a_1^2, \text{ or than } \left(\frac{12}{a_1'^2}-\frac{7}{a_1''^2}-\frac{5}{a_1^2}\right)a_1^2,$$

where $a_1 a_1' a_1''$ are the least, mean, and greatest axes of the first sphere, the stable positions will be different. Thus the stable positions will always be with major axis of the second in the line of centres if c^2/a_1^2 is greater than 7/5.

The "functions of the second kind," which are the two remaining solutions of the differential equation of the fourth order satisfied by u_{sr} v_{sr}, are also briefly investigated.

IV. "On the Measurement of the Magnetic Properties of Iron." By THOMAS GRAY, B.Sc., F.R.S.E., Professor of Dynamic Engineering, The Rose Polytechnic Institute, Terre Haute, Indiana. Communicated by Lord KELVIN, P.R.S. Received April 6, 1894.

(Abstract.)

This paper gives the results of a continuation of the investigation which formed the subject of a paper communicated to the Royal Society in 1892, and published in the 'Philosophical Transactions,'

vol. 184, A, pp. 531—542. The results now given have been to a large extent obtained by the same method, namely, from the curves giving the relation of the current flowing in the circuit to the time measured from the application or the reversal of the impressed E.M.F. on the circuit. In this case, however, the personal element has been eliminated from the curves by the application of the autographic recorder referred to as under construction in the previous paper. This apparatus, which is a modification of the "Thomson siphon-recorder," has been found to work satisfactorily, and has considerably increased the ease and the accuracy with which the curves can be produced. A description of the apparatus and specimens of the curves drawn by it are included in the paper. There is also included in this paper a description of the apparatus and method of experiment in the application of a wattmeter to the determination of the energy dissipated by transformers under E.M.F.'s of different frequency of alternation. The accuracy of the measurements so made were checked by comparison with the results of measurements made by Joubert's instantaneous contact method. The apparatus and method of experiment adopted for the application of this method were to some extent different from those commonly employed, and they are therefore described.

The results of some further experiments on the large electromagnet used in the previous experiments, and described in the paper above referred to, are given, but a large part of the results quoted in this paper refer to closed circuit transformers of the types manufactured by the Westinghouse and the General Electric Companies. The experiments have been chiefly directed to the following points:—

1. *A Comparison of the Total Energy required to produce Different Magnetic Inductions, and the Corresponding Dissipation of Energy.*—In connection with this, the effect of air gap in the magnetic circuit has been investigated somewhat more fully. It is shown that, by introducing a moderate air gap, the energy dissipated for a given induction through the coils may be reduced one-third.

2. *The Law of Variation of Hysteresis with Variation of Induction.*—The experiments indicate that, although for any special case the energy dissipated can be approximately expressed by an equation of the form $E = AB^{\alpha}$, that both A and α are different for different kinds of iron. It seems probable, also, from the results obtained, that α is not absolutely constant for any one iron, but that it increases with increase of B.

3. *The Effect of Increased Frequency of Cyclic Variation of Magnetism on the Dissipation of Energy.*—In this investigation a transformer, the iron case of which was made up of very thin sheets, was used. The thickness of the sheets was about 16-100ths of a millimetre, and the sheets were insulated from each other by means of thin

paper. The full load capacity of the transformer was about 6,000 watts. The range of frequency (including the autographic recorder, the wattmeter and the Joubert's instantaneous contact method experiments) was about from 3 per minute to 8,000 per minute. The results indicated that, throughout this range, there is no variation in the dissipation of energy per cycle when the inductions are equal.

Data deduced from these experiments as to the magnetic qualities of the iron used in the different transformers are given in the paper.

V. "On the Influence of certain Natural Agents on the Virulence of the Tubercle-Bacillus." By ARTHUR RANSOME, M.D., F.R.S., and SHERIDAN DELÉPINE. Received May 1, 1894.

Three years ago Dr. Ransome communicated to the Society the results of some experiments, carried out in concert with Professor Dreschfeld, of Owens College, "On certain conditions that modify the virulence of the bacillus of tubercle."

The tendency of these researches was to prove "that fresh air and light, and a dry and sandy sub-soil, have a distinct influence in arresting the virulence of the tubercle-bacillus; that darkness somewhat interferes with this disinfectant action; but that mere exposure to light, in otherwise bad sanitary conditions, does not destroy the virus."

The following table gives the results of similar experiments by ourselves.

Table I.

Experiment No.		
3.	1.	Rabbit inoculated in peritoneum with fresh sputum. Killed 55 days after. Showed well-marked tuberculosis.
7.	2.	Rabbit. Sputum exposed to light and air 45 days in June and July. Showed no tuberculosis after 86 days.
8.	3.	Rabbit. Sputum exposed in air-shaft in dusk at the same time. Showed slight tuberculosis after 86 days.
11.	4.	Guinea-pig. The same sputum exposed at the same time, in air and light, inoculated under the skin. Showed no distinct tubercle in 80 days.
12.	5.	Guinea-pig. Same methods, only in dusk. Showed advanced tuberculosis in 80 days.
58.	6.	Guinea-pig. Another sputum exposed in April for 16 days to little or no air, in darkness. Gave well-marked tubercle after 42 days.
59.	7.	Guinea-pig. Ditto, ditto.

We have now carried the enquiry a little further, and, amongst other objects, have endeavoured to determine how short a period of exposure to air and light would suffice to destroy the poisonous action of the microbe. We selected guinea-pigs as the most susceptible animals to test this question.

In the first instance pure cultivations of the bacillus were prepared, and were found to be active by frequent inoculations. Small portions of this material were spread in a thin layer, upon pieces of sterilized paper. They were arranged in circles of about 2 mm. in diameter, so as to give every opportunity for the action of the elements. They were then exposed in a glass-room, with free access to air and light, *i.e.*, close to open windows, for diminishing periods of time, viz., 14, 10, 6, 4, and 2 days respectively. Contemporaneous daily records were kept of temperature, maximum and minimum, and of the amount of sunshine taken through the glass roof, by means of one of Negretti and Zambra's sunshine recorders.

The following table (Table II) gives the results of the meteorological observations, but as will be seen presently, only those for the first few days are of importance.

No result from the other papers; the control experiments showing that the bacilli used after this date had lost their virulence. Even the results of Experiments 97 and 98 are doubtful on that account, but Experiment 85 was made with a very virulent specimen, as was proved by the inoculation of two other guinea-pigs, with paper infected with the same quantity of the same cultivation, and kept the same length of time, but not exposed to sunlight. In both these cases advanced tuberculosis was produced in 44 days.

It may be noted that only one of these experiments can be entirely relied upon, and that in this case, after 4 days' exposure to air and 12¼ hours of sunshine, there was no result from the inoculation.

These observations, though not in any way conclusive, are in accord with those of Professor Koch,* and they encouraged us to believe that even short exposures of the tubercle-bacillus, even in sputum, to air and light, might render it innocuous.

In the next series of observations, it was determined to allow

* Koch, 'Verhandlungen des Internationalen Medicinischen Congresses' (Berlin, 4th to 9th August, 1890), vol. 1, p. 35. Koch says that for some years it has become known that light could kill bacteria. He alludes, no doubt, to the experiments of Downes and Blunt, Arloing, Roux, and others. Marshall Ward's experiments with the *Bacillus Anthracis* are still more recent. Koch had been able to confirm this with regard to the tubercle-bacillus, of which cultivations exposed to sunlight might be killed in a space of time varying from a few minutes to some hours. When exposed to diffuse daylight in a room they were killed in from five to seven days.

Table II.

Date.	July 20	21	22	23	24	25	26	27	28	29	30	31	Aug. 1	2
Temperature { Maximum......	96°	103°	95°	95°	96°	72°	70°	100°	98°	93°	93°	110°	105°	107°
Temperature { Minimum......	50	50	50	50	60	53	52	48	56	54	52	50	55	55
Hours of sunshine	3	2	7¼	1 hour weak shine	2¼	3½	3¼	1	1½	1¼	4½	2½	1¾	1¾
Total sunshine	12¼				5¾									
Results	*Exp.* 85.—4 days exposed. No tuberculosis in 43 days.				*Exp.* 98.—2 days exposure. Local swelling disappeared in 10 days. No result in 78 days.									
	Exp. 97.—Exposed 6 days. No result in 150 days.													

tuberculous sputum to dry (*a*) in air and light, (*b*) in air and darkness, (*c*) in a close cupboard.

Fresh sputum, rich in bacilli, was obtained and exposed in watch glasses. Specimen (*a*) was dry in four days; specimen (*b*) in eight days; and specimen (*c*) in 19 days.

Specimens (*a*) and (*b*) were closed up as soon as they were dry and kept until specimen (*c*) was ready, and then portions of the sputum were scraped off the glasses and inoculated into guinea-pigs directly after scraping. Table III gives the results.

Table III.

No.		
117.	1.	Sputum (*a*) inoculated subcutaneously into two guinea-pigs,
118.	2.	killed 64 days afterwards, gave well-marked tuberculosis.
119.	3.	Sputum (*b*), similarly used, showed no results
120.	4.	53 days after.
126.	5.	Sputum (*c*) gave well-marked tuberculosis 50 days afterwards.

The results of these experiments are somewhat anomalous. The sputum was in rather thick masses and thus dried slowly, and would with difficulty be affected by the natural agents to which they were exposed. This fact would probably account for the continued virulence of sputa of 1 and 2, but the immunity from sputa 3 and 4, after eight days exposure to a current of air in darkness, is hardly likely to be due to this exposure; we can, therefore, draw no decided conclusion from this series of experiments.

In the fourth series of observations, the sputum was spread upon paper, and was thus more rapidly dried at the ordinary temperatures, about 24 hours sufficing. It was then in most cases scraped and thus partly converted into "tuberculous dust" before being exposed to the same conditions as before. In this way it might be expected to be more readily affected by the elements.

An attempt was made to measure the amount of air as well as light, an anemometer and a sunshine-recorder being placed near the sputum exposed at the open window.

Only a rough guess could thus be made as to the quantity of air passing over the sputum, however, for the papers had to be loosely covered with thin gauze to prevent the "dust" from being carried away by the wind, and the anemometer recorded currents in both directions.

Three sets of experiments were made.

1. Papers were placed, to be used for control experiments, in the dark, close cupboard.

2. Papers were placed in the air-shaft of a draught-closet in dim light, pure air only passing through it.

3. Papers were exposed to air and light for three days, February 20, 21, and 22. The rate of air current was about 1,000 ft. per hour, and the sunshine recorded was one hour. Others were exposed for a longer period.

The amount of tuberculous dust was so small that portions of the paper were inserted together with it, under the skin.

Table IV.

First Set of Experiments.

No.		
151.	1.	Sputum kept only one day in a closed, dark cupboard, after drying on paper, produced well-marked tuberculosis in 31 days.
191.	2.	Sputum kept under the same conditions, but exposed to a little air for 35 days, produced distinct local tuberculosis in 23 days.
192.	3.	*Idem.*
		Second Set of Experiments.
160.	1.	Sputum kept in the draught closet for three days in a current of air (about 1,000 cubic feet per hour) in darkness, at the ordinary temperature, gave well-marked tuberculosis in 32 days.
170.	2.	Sputum under exactly the same conditions gave well-marked tuberculosis in 24 days.
		Third Set of Experiments.
156.	1.	Sputum exposed to light for three days, during which there was one hour of sunshine. Ventilation good. Temperature, maximum 50° and minimum 38° F. No tuberculosis after 46 days.
157.	2.	Sputum under the same conditions as the last, except that it had not been reduced to dust, gave the same negative results after 50 days.
189.	3.	Sputum exposed to light for seven days; 15 hours of sunshine; brisk ventilation. Temperature, maximum 88°, minimum 29° F. No tuberculosis after 22 days.
190.	4.	Sputum exposed to light for two days (after being kept dry for four weeks); short exposure to sunshine (not many hours); ventilation slight. Temperature, maximum 60°, minimum 22°. No tuberculosis after 22 days.

It will be noted that in all the specimens exposed in the dark, tuberculosis was the result, but it must be observed, that in the case of those exposed in the draught-closet, only three days were allowed to pass before they were removed from the influence of the air-current. On the other hand, all the specimens exposed to both air and light, whether for two, three, or seven days, were found to have entirely lost their power for evil.

The specimen exposed for two days only, had, however, been kept for four weeks before being exposed to these influences, and it had thus lost a portion of its virulence.

These researches have an important bearing upon the question of the limits of the infectiveness of tubercle.

It has long been known that the disease is most common in the dirty, ill-drained, ill-ventilated dwellings of the poor, and, even in records intended to prove the contagiousness of phthisis, there are few, if any, of transmission of the disease in clean, well-lighted, well-ventilated houses or hospitals, even those for consumption. Long before Koch's discoveries, and before the disinfection of sputum was practised as it is now, the conveyance of the disease, under these conditions, was recognised by many to be one of the rarest events.

If the results that we have obtained with sputum are confirmed by others, as we trust they will be, they will afford some explanation of these facts.

So far as they extend at present, they show (1) that finely divided tuberculous matter, such as pure cultures of the bacillus, or "tuberculous dust," in daylight, and in free currents of air, is rapidly deprived of virulence, (2) that even in the dark, although the action is retarded, fresh air has still some disinfecting influence, and (3) that in the absence of air, or in confined air, the bacillus retains its power for long periods of time.*

VI. "On some Voltaic Combinations with a Fused Electrolyte and a Gaseous Depolariser." By J. W. Swan, M.A. Communicated by Lord Rayleigh, Sec. R.S. Received February 28, 1894.

It is well known that fused salts behave in many respects like electrolytes in solution, and that voltaic combinations analogous to well-known voltaic cells may be formed with fused electrolytes.

The experiments of Brown† have recently illustrated this subject in relation to the Daniell type of cell. For various reasons it appeared to the writer desirable to ascertain the behaviour of a cell with fused electrolyte and a gaseous depolariser, and corresponding in this last particular to the Upward cell.

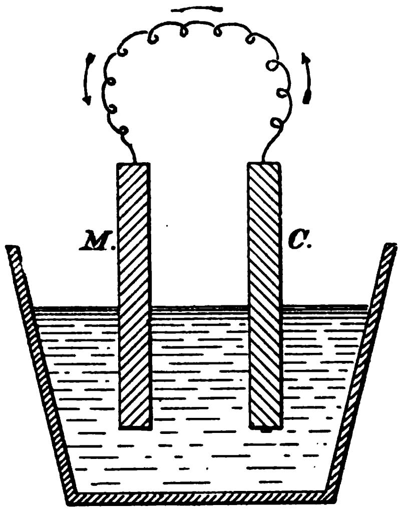
The following is chiefly a record of some of the experiments made in connection with this research.

A cell of this kind may be looked at from a theoretical point of view as follows:—A rod of metal, M (fig. 1), is immersed in a fused chloride of the same metal, MCl, and a chemically inactive conductor, C, is also immersed in the fused salt; when M and C are connected with an electrostatic volt-meter, the metallic chloride is immediately

* A portion of the expenses of this research has been defrayed by a grant from the British Medical Association.

† 'Roy. Soc. Proc.,' vol. 52, pp. 75—91.

Fig. 1.

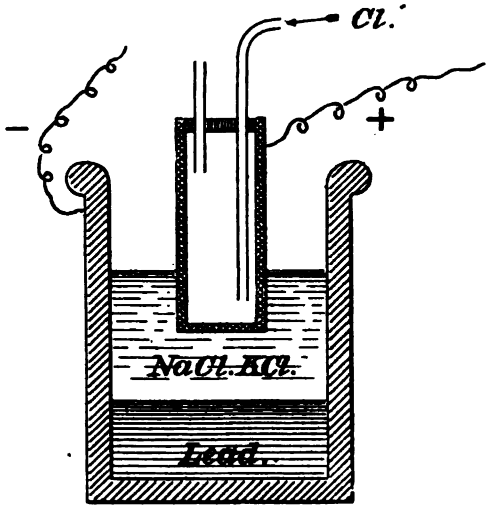


polarised, and an E.M.F. calculable from the heats of combination, M/Cl = MCl, is developed. If M and C are metallically connected, a momentary current passes, but the combination is immediately polarised by the opposing couple, formed by the cathion of the electrolyte M and the pole C. To prevent this polarisation, chlorine has to be supplied at this pole. Complete depolarisation should occur if the pole C consisted of a solid rod of chlorine. This is impossible, but gaseous chlorine, used as a depolariser, can be made to effect more or less complete depolarisation, and should, theoretically, yield as the result of its heat of combination with lead an E.M.F. of 1·7942 volts. In experiments made with a view to realise as nearly as possible the ideal condition for preventing polarisation the cathode was always molten lead. It was found that hard gas-retort carbon had very little action upon molten alkaline chlorides and on chloride of lead, at the temperature required for their fusion. Carbon was, therefore, employed as the anode or conducting pole in most of the combinations.

The electrolyte used was either the molten chlorides of sodium and potassium mixed, or chloride of lead. As there is a continuous formation of $PbCl_2$ during the action of the cell, and as it is a good conductor, it alone was finally adopted as the electrolyte. As a depolariser, chlorine gas was used. Many experiments were made to find a suitable way of applying the chlorine. The following are details of some of the most suggestive of them.

Exp. 1.—A cell was constructed as shown in fig. 2. The arrangement consists of an outer iron vessel, with a stratum of molten lead

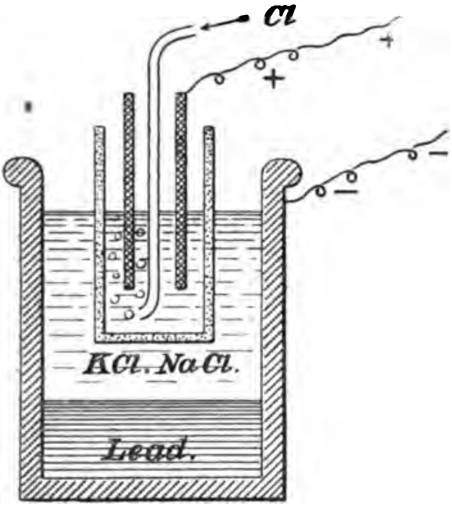
Fig. 2.



covering the bottom to some depth, over the lead is a layer of NaCl and KCl previously fused, into which is immersed the lower and closed end of a carbon tube, which forms the + pole. The mouth of the carbon tube is closed by a fire-clay lid luted on, and through which pass two small clay tubes for the inlet and outlet of chlorine. The whole was heated in a small gas furnace. A binding screw on the iron vessel, which served as a connection with the lead, was used as the negative terminal, and another screw fixed on a copper ring surrounding the carbon tube, served as the positive pole connection. The first trial was made without chlorine. Short circuited through 1,000 ohms the cell developed an E.M.F. of 0·3 volt. A momentary current of more than one ampère was observed when the cell was short circuited through a low resistance ammeter. Chlorine was then passed through the tubes inside the carbon pole, but *no depolarising effect* was observed, even when the chlorine had a slightly higher pressure than the atmosphere, yet the gas passed through the exposed sides of the carbon tube and through the cement at the top. This experiment was repeated several times with carbon tubes of the smallest possible thickness, and always with the same result. It is evident, therefore, that an absorption of chlorine similar to that which takes place in the Upward cell does not occur when a molten electrolyte of the kind employed in this experiment is used.

Exp. 2.—As this method of applying chlorine was unsuccessful, another form of apparatus was adopted. The poles were of the same material as in the previous experiment, but the carbon pole

FIG. 3.



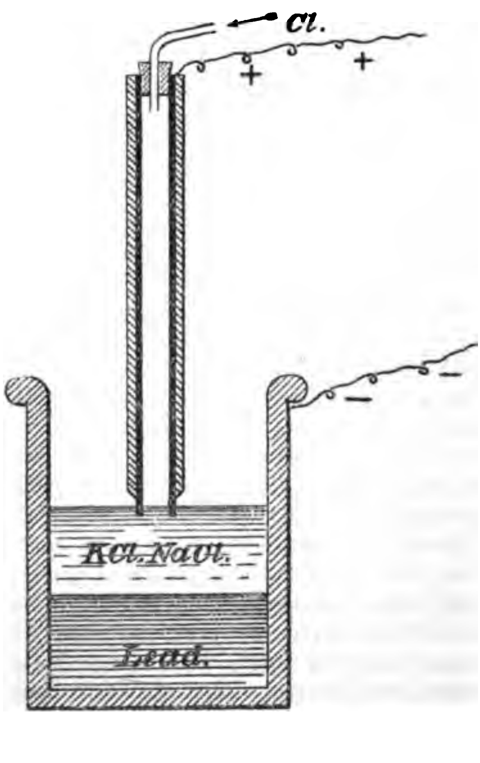
was an open tube. The electrolyte was a fused mixture of equivalent proportions of NaCl,KCl. A porous pot was introduced in order to separate the products of electrolysis set free at the electrodes. The chlorine gas was supplied through a clay tube, which passed down the centre of the carbon tube. As before, an iron crucible was used as the containing vessel for the fused lead and the electrolyte, it also served as a means of electrical connection with the lead. In relation to the depolarisation effect of the chlorine, which it was the principal object of the experiment to observe, the interfering action of the iron was found by comparison with porcelain to be practically *nil;* this no doubt is a consequence of its becoming coated by local action with a film of lead. The whole arrangement was heated in a reverberatory furnace. When the electrolyte was perfectly fused, the element was short circuited through a volt-meter of 1000 ohms resistance. An E.M.F. of 0·3 volt was observed, the outside current being from the carbon to the lead. This was the E.M.F. after polarisation. A current of chlorine was then passed through the earthenware tube; while the current of gas was slow there was no effect, but when the speed of the issuing gas was increased until the gas passed in bubbles along the side of the carbon, alternately surrounding it with chlorine and electrolyte, the E.M.F. rose to 1·25 volt. The action of the cell was then similar to a completely depolarised cell. When short circuited through a low resistance ammeter, it produced a steady current of 1·0 ampère for three-quarters of an hour. The potential difference between the poles was of course very small, while this current was passing, the exterior resistance being very small com-

pared with the interior. When, however, the circuit was opened, it almost instantly rose to 1·25 volt.

So far the experiments showed that, as chlorine is nearly or perfectly insoluble in fused chloride of lead, or in fused chlorides of sodium and potassium, *it is necessary in this case that the surface of the carbon pole on which the cathion is deposited be alternately exposed to the action of the gas and electrolyte.* Many experiments confirmed this conclusion. The exposed surface of the carbon tube in this experiment amounted to only 10 or 12 sq. cm.

Exp. 3.—As in the arrangement last described, the use of a porous pot and a clay tube was found to be objectionable, through the action of the electrolyte upon them, an arrangement was devised by which the use of the porous pot and tube were avoided. The details are seen in fig. 4. The carbon tube serves as an electrode, and also for conveying the chlorine to the electrolyte. To render it impervious to the gas, it was surrounded by a close-fitting porcelain tube. This tube was closed at the top by a paraffined cork, through which a glass tube in connection with the chlorine supply was passed. The remainder of the apparatus was the same as in Experiment 2, but

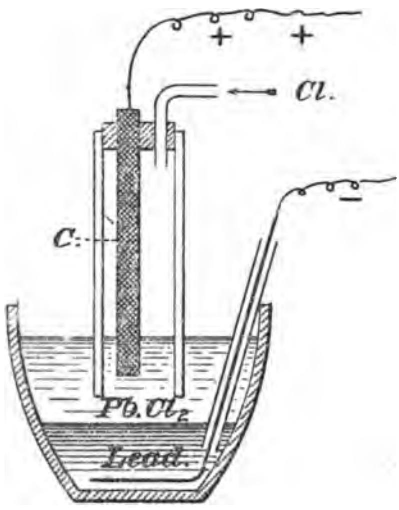
FIG. 4.



without the porous pot. This arrangement gave the following results. The E.M.F. when short-circuited through a voltmeter (300 ohms) gave 1·4 volt, the chlorine entering rapidly. When short-circuited through 1 ohm, it gave a constant current of 0·6 ampère with a P.D. of 0·9 volt. The rather large interior resistance of 1·3 ohm is due not to the electrolyte, but to the greater length of the carbon tube, and bad contacts produced by the corrosive action of the chlorine. No good results were obtained until the chlorine gas bubbled out of the carbon tube, thus realising the conditions before mentioned, as necessary for the production of any large electrical effects.

Exp. 4.—With a view to obtain larger effects, another form of cell was tried, as shown in fig. 5. The carbon pole C consisted of a thin rod of electric light carbon, 5 mm. diameter and 15 cm. long. It was passed

FIG. 5.

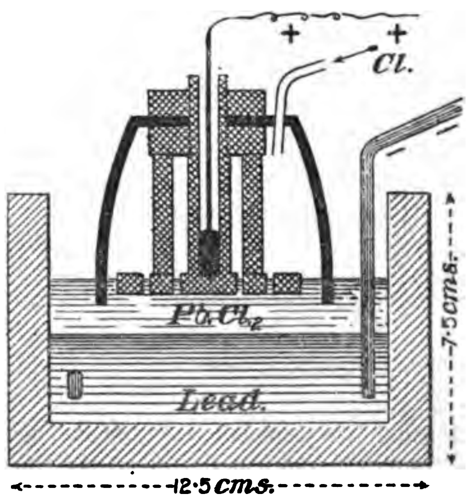


through a cork fitted in a porcelain tube, 2·5 cm. diameter. A glass tube bent at right angles was passed through the same cork, to serve for the delivery of the chlorine. The end of the carbon rod was a little short (about 3 mm.) of the end of the porcelain tube. The containing vessel was a Berlin porcelain crucible, 7 cm. diameter. The conductor from the lead was an iron wire, protected from the action of the electrolyte by a surrounding porcelain tube. The electrolyte was chloride of lead. The whole was arranged as shown in fig. 5. When the chlorine issued from the porcelain tube, the necessary conditions for depolarisation were in a large degree realised, the contact between the electrolyte and the carbon being at times almost broken,

and at other times the chlorine forming a nearly complete envelope round the carbon, these conditions following in rapid alternation. The highest E.M.F. observed was 1·25 volt. The largest current given was 0·9 ampère with a P.D. of 0·25 volt. The current was fluctuating, owing to the varying conditions at the carbon pole.

Exp. 5.—The following figure shows a construction almost identical with the last, differing only in a few practical details, occasioned by this cell being made larger than the last. The porcelain tube previously used was replaced by an inverted porcelain crucible, having two holes drilled in the bottom; the larger hole afforded a passage for the conductor from the carbon pole: through the smaller one there passed a porcelain pipe for the chlorine supply. The carbon pole was composed of a disc of gas retort carbon, pierced with holes, as shown in the figure. In the middle of the disc was screwed a tube of carbon, which passed up through the larger hole in the bottom of the crucible and was secured in this position by nuts of retort carbon. The bottom of the carbon tube was filled with fused lead, into which dipped a thick copper wire that formed the positive pole connexion.

FIG. 6.



The connexions between the carbon and porcelain were luted with a mixture of borax and fire-clay fused at a bright red heat. The other vessel consisted of a short cylindrical plumbago crucible. The fused lead was, as usual, on the bottom, connexion being made with it by means of a heavy open iron ring, with its free end turned up and bent over the rim of the crucible. The internal resistance was small, owing to the improved contact of the leading wire with the carbon

pole. The electrolyte was fused $PbCl_2$. The whole was heated in a gas furnace. When the electrolyte was fused, the chlorine was passed rapidly through, so that it issued from under the porcelain crucible. The E.M.F. was then between 0·94 and 0·96 volt., and never rose higher than 0·98 volt. The lower E.M.F. was evidently due to the fact that part of the surface of the carbon pole was not subject to the action of the chlorine, but remained polarised by deposition of lead. The behaviour was much like that of a constant cell with an E.M.F. of between 0·94 and 0·96 volt. The method of observation was to alter the exterior resistance and then read the current and P.D., then break the circuit and read the E.M.F.

P.D. (In closed circuit). Volt.	y. Ampères.	Calculated internal resistance. $\frac{\text{E.M.F.}}{y} - \frac{\text{P.D.}}{y}$ (calculated). Ohm.
0·26	12·0	0·06
0·24	10 0	0·07
0·62	4·0	0·08
0·76	1·26	0·16
0·72	1·75	0·14
0·72	2·15	0·12
0·66	2·50	0·12

From these observations it will be seen that the internal resistance was calculated, col. 3, in order to find whether polarisation is greatest when a small or large current is taken from the cell. From the results it is apparent that the internal resistance, and at the same time the polarisation, *decrease* when the current *increases*. This kind of cell, therefore, differs from those in which aqueous electrolytes are used, inasmuch as the polarisation decreases with increased electrical output. The observations of P.D. and E.M.F. were taken almost simultaneously, and the variation of resistance as the gas bubbles passed out was thus avoided. As the internal resistance was very small the whole time, and remained almost constant during a variation of the current from 1·26 to 2·5 ampères, it may be said to be a constant battery, with an E.M.F. lower than the theoretical value. The reason of this lower E.M.F. is probably due to some part of the large carbon plate being covered with reduced lead, thus forming an opposing couple of smaller capacity and lower resistance than the primary elements, its effect being to reduce the main current. The E.M.F. of this opposing couple is of necessity the same as that of the main current, but, owing to its lower internal resistance, its P.D. is less; if it were not so, the cell would yield no appreciable current. This reasoning explains why the results obtained with small cells were better than those obtained with large ones.

Besides the experiments mentioned, trials have been made, with

more or less success, of many other forms of this combination, including some in which very porous hollow carbon poles were used, and through which the chlorine was forced, but the effects obtained were less than those recorded. The research has proved that it is possible to form pyro-batteries of the Upward type, although it is extremely difficult to realise the conditions required for effective action. In a future communication I hope to record the results of experiments made, with a view to utilise oxygen as a depolariser in connexion with cells with fused electrolytes.

VII. "Measurements of the Absolute Specific Resistance of Pure Electrolytic Copper." By J. W. SWAN and J. RHODIN. Communicated by Lord RAYLEIGH, Sec. R.S. Received February 28, 1894.

At the beginning of 1893 it was resolved to make some very careful measurements of the specific resistance of pure electrolytic copper, drawn into wire without previous fusion. Researches made during the latter end of 1892 had shown that the specific resistance of electrolytic copper varies considerably. The resistance of about thirty wires of the same length and diameter, made from specimens of electrolytic copper, prepared in different ways in the laboratory, showed differences of resistance amounting to a maximum of 1·4 per cent., both when in a hard and when in a soft or annealed state, and measured at the same temperature.

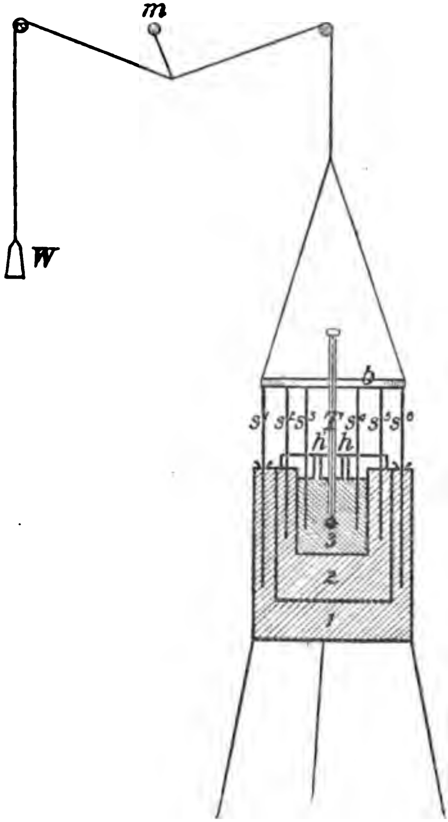
These preliminary measurements were made by means of a Wheatstone's bridge, constructed for comparing the unknown resistances of short well-conducting wires with the resistance of a standardised platinoid wire, according to Thomson's method. The accuracy obtainable by this method was 0·25 per cent. The best specimens of wire were subjected to a further and still more accurate examination.

The measurements of the specific resistance and temperature coefficient of one of these wires, and of some wire made from the same copper, after undergoing a second electrolytic refining, form the subject of this paper. It was resolved to make measurements giving an ultimate accuracy of 0·1 per cent. As they were intended to be absolute, the first problem was the determination of the exact dimensions of the wires to be measured. The measurement of the length was made by means of direct comparison with a standard metre rule; that of the diameter was determined by the specific gravity method, which consists in finding the absolute weight of a known length of wire and its density or unit volume weight as determined from its specific gravity, and then calculating its average diameter.

In the determination of the specific gravity, both the hydrostatic balance method and the picnometer method were used. The latter method was found to give more accurate results. A point of great importance was the estimation of the temperature of the specimens during measurement. To obtain as great accuracy as possible in the temperatnre readings, an apparatus similar to a calorimeter was employed for enclosing the coil of wire whilst it was measured. A standard thermometer divided in tenths of degrees centigrade was used, placed in the vessel containing the sample of wire. It was read at a distance by means of a telescope.

The arrangement is represented by fig. 1. It consists of three

FIG. 1.

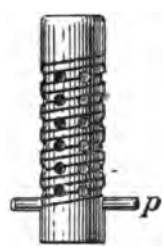


cylindrical tinned iron vessels arranged concentrically one inside the other. The section of the two outer vessels (fig. 1) is distinguished by the shading from that of vessel 3. The vessels formed three water-

tight compartments. Jackets 1 and 2 were filled with water, and the vessel 3 with paraffin oil having a flashing-point of 150° C. To stir the liquids, three circular rings of iron wire were enclosed, one in each of the compartments. These rings were suspended on the iron wires, s^1, s^2, &c. The iron wires themselves were fixed on a bar of brass, *b*. By means of the crank of an electromotor (*m*) and strings and pulleys, the bar *b* could be moved up and down, stirring each of the liquids simultaneously. To make the motion easy a balance weight, W, was used. The coil to be measured was enclosed in compartment 3. This compartment was closed at the top by a hollow lid of tinned iron, pierced with holes to allow for the passage of the stirring rods, &c. This lid effectually protected the paraffin oil from surface cooling. The two holes, *h* and *h'*, made to allow the wires connecting the ends of the coil to pass out, were lined with ebonite to prevent contact with the metal of the lid. The thermometer, T, was let down through a tube in the middle of the lid. As a proof of the effectiveness of the arrangement, it may be mentioned that a small Bunsen burner when burning at its full power, and placed under compartment "1" raised the temperature of the inner one "3" only 0·1° C. in thirty seconds, a length of time more than sufficient for making a resistance determination. When the temperature in "3" had been raised to 92° C. and then allowed to cool (being constantly stirred), the temperature (in "3") only fell to 40° C. in twenty-four hours, notwithstanding the temperature of the laboratory was only 15° C.

Another important detail was an arrangement for securing the wires whilst they were measured. Fig. 2 represents this. It consists

FIG. 2.



of a piece of ebonite tube 5 cm. diameter, with a deep double screw thread cut on the outside. It was pierced all over with large holes 1 cm. diameter, to allow the paraffin oil to freely circulate in the inside, where the thermometer was inserted. A short rod of ebonite (*p*) was put through and across the cylinder at the bottom. The wire to be measured was bent round this cross rod, so that equal

lengths were hanging down. The double bent wire was then wound up in the double screw thread, and secured at the top by means of string; the influence of self-induction was thus avoided. Each wire had four terminals, the main current terminals, and the shunt terminals, which were soldered to pieces of stout copper, the distance between which determined the length of the wire under examination. These reels saved the wires from being hardened or otherwise injured. The length of the wire was taken both before and after the electrical measurement. In some earlier experiments when mica strips were used for coiling the wire upon, great differences were observed. When the ebonite reels were employed hardly any difference could be observed.

The electrical measurements were made by the fall of potential method, refined as much as is possible. A D'Arsonval galvanometer was employed as an indicator of potential difference. The "dead beat" property of this instrument is an advantage which in this class of measurement cannot be over-rated. Finding in the first experiments with the D'Arsonval galvanometer, that its sensitiveness was not sufficient for our purpose, the upper suspension was lengthened very considerably. The coil was made to hang on the upper wire, the wire below the coil being left slack. By this means the required degree of sensitiveness was obtained. The galvanometer readings were made by means of a telescope and scale, a plane mirror being attached to the galvanometer coil. To gain the advantage of using very small deflections, the scale and galvanometer were widely separated, the distance between them was 8 m. The largest deflections used were 600 divisions of the scale (each division = 1/30 in.), corresponding to (at that distance from the mirror) an angle of 1° 49′, which falls within the limits for:—

(1.) $\tan 2\text{ angle} = 2 \tan \text{angle} \pm 0{\cdot}001 \times 2 \tan \text{angle}.$

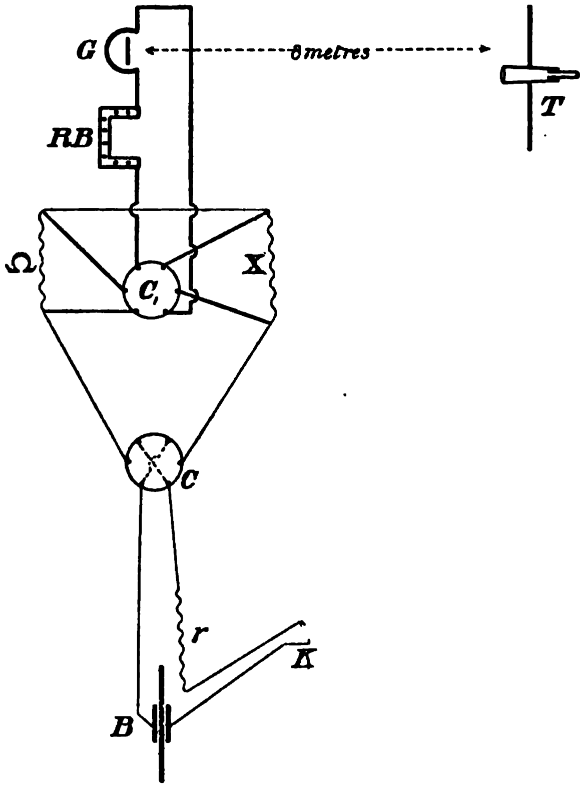
The readings of the deflections were found to be proportional to the P.D. by direct and carefully made measurements, they were always read in two directions, and the sum taken as the deflection; the accuracy was thus doubled, and possible error by displacement of the zero point avoided.

The limit of sensitiveness of the galvanometer was:—

1 scale division deflection = $\frac{1}{30000000}$ ampère (approximately), or = 0·00000003101 ampère (accurately).

The resistance of the suspended coil was 10 ohms. The time required for the coil to come to rest was about two seconds. The accompanying diagram shows the arrangement for making the measurements. They were made by *direct comparison* between a standard ohm and the various coils to be measured, and in such a manner that error due to the resistance of the leads was eliminated.

Fig. 3.



The following is a description of the details:—B was a single lead and lead peroxide element, of a comparatively large capacity (30 ampère hours). It was found necessary to have the capacity of the cell tolerably large to avoid the necessity of making too large corrections for the fall of potential in the cell itself between different observations. By means of a mercury contact key, K, the battery could be short-circuited through a nickel wire resistance r (of about 6 ohms), the standard ohm R and the resistance to be measured X. All these resistances were in series. (The current in the main circuit was only 0·2 or 0·25 ampère, and did not produce any sensible heating of the various resistances.) By means of the current reverser, C (a mercury switch), the current direction could be easily changed. The galvanometer, G, was connected with another mercury switch, C_1, by means of which it could be put in shunt to the standard ohm and X alternately in rapid succession. In the galvanometer circuit the resistance box, RB, was inserted in order to render it possible to make the deflections nearly equal when X differed considerably from R.

The method of making a determination was as follows :—1st, C_1 was placed in the position which made the galvanometer circuit a shunt of the standard ohm. RB was adjusted so as to make the deflection of a convenient amount by placing in a resistance R_r (R_r having been determined by means of preliminary experiments). Then K was pressed down and a deflection of the galvanometer "a" observed. The current was then reversed, and a deflection "b" observed. $(a+b)$ was then placed in the observation table under the heading G_R. The key opened. The temperature of R observed and noted (T_R). 2nd, C_1 was then placed in the position which made the galvanometer circuit a shunt to X. RB adjusted to make the deflections of a suitable size by putting in a total resistance of R_X (determined by previous experiment). Then K was pressed down, and the deflection a_1 observed, the current was then reversed, and a deflection b_1 observed. (a_1+b_1) was then put down in the table under the heading G_X. The temperature of X observed and noted (T_X).

3rd observation. No. 1 repeated.

4th " " 2 "

A set of measurements like these were made in 20 seconds, thanks to the dead-beat quality of the galvanometer, and the easy manipulation of the mercury switches. It is evident that, *if the galvanometer deflections were exactly proportional to the current, and if the resistance of the galvanometer circuit was sufficiently high to be neglected, and also if the potential difference of the accumulator did not alter between observations* 1 *and* 2, the resistance X can be put

$$(2.)\qquad X = \frac{G_X(R_X+S)}{G_R(R_R+S)}R.$$

S = resistance of the galvanometer + resistance of galvanometer leads.

The proportionality of the galvanometer deflections with the limited range employed was, as previously stated, experimentally proved. The conductivity of the galvanometer as a shunt was negligible, as is evident from the fact that it was more than 2000 ohms resistance (when a minimum), and then diminished the total resistance of the main circuit about 1/8000 ohm (it was shunted round $\frac{1}{4}$ ohm), the resistance of the main circuit amounting to 8 ohms. The fall of potential of the accumulator was very seldom appreciable during one set of measurements. This will be seen from the identity between observations 1 and 3 and 2 and 4 respectively in the tables. When appreciable a correction was applied. When all the arrangements were completed, a determination of the specific resistance of the best specimen of copper (marked "A") was made.

Origin of the Copper.—The copper was deposited in a large rocking

Table I.—Specific Gravity of the Rocking Tank A Deposit. Temperature 15° C.

Description of specimen.	Weight in air + tare.	Tare.	Weight in air.	Weight in H_2O + tare.	Tare.	Weight in H_2O at 15° C.	Repetition of weight in air.	Specific gravity.
	grams.	grams.	grams.	grams.	grams.	grams.	grams.	
I. Pieces of the deposit as it came out of the bath.	103·5635	2·2548	101·3087	90·1530	0·1610	89·9920	101·3080	8·9521
II. Another piece of the same	95·1994	2·2550	92·9444	82·7217	0·1610	82·5607	92·9445	8·9500
III. Half of I hard drawn	26·7334	2·2551	24·4783	21·9176	0·1746	21·7430	24·4783	8·9491
IV. Half of I hard drawn and afterwards annealed in CO_2 gas	27·5580	2·2551	25·3029	22·6514	0·1746	22·4768	25·3028	8·9533

tank from ordinary sulphate of copper solution prepared from pure crystallised sulphate of copper, pure sulphuric acid, and distilled water. The anode was a large plate of ordinary commercial electrolytic copper, and the cathode was a large polished plate of rolled copper. Before placing the cathode in the bath it was silvered by rubbing it over with a solution of cyanide of silver in potassium cyanide. This coating of silver was converted into iodide of silver by means of a solution of iodine in potassium iodide. As is well known, this treatment renders the stripping of the deposit from the cathode an easy matter. On this cathode copper was deposited to a thickness of 2·5 mm., and then the deposit was stripped off. Several deposits were made and tested roughly, as stated in the beginning of this paper. The best of them was one marked "A."

Preparation of the Wire.—A strip was cut from the deposited sheet of copper, filed round, and then drawn through sapphire dies to a diameter of approximately 0·02 in.

These determinations were made by means of the hydrostatic balance principle. A re-determination of the specific gravity of the copper I in the above table was made by means of a picnometer.

Picnometer	+ distilled H_2O at 15° C.	=	91·2878 grams.
„ + specimen +	„ „	=	123·3562 „
Specimen	....	=	36·1012 „
∴ weight of displaced H_2O	....	=	4·0328 „

Specific gravity = 8·9519.

As is seen from the above numbers, the specific gravity of copper when it is pure varies very little with hardness and other conditions, the variations when at a maximum only amounting to 0·0004 of the whole. The mean of the above results may, therefore, be taken as the specific gravity of this copper at 15° C. The mean is

8·9511 = the specific gravity.

This value is not the one required for the calculation of the dimension of wires; what is required is the density or absolute weight of an ideal cubic centimetre of the metal at 15° C. The weight of 1 c.c. of water at 15° C. is, according to Kohlrausch, 'Praktische Physik,'

0·99915 gram.

The specific gravity of the copper divided by this figure gives the density:—

Weight of 1 c.c. of copper at 15° C. = 8·9587 grams (8·959).

Specific Resistance of Deposit "A" (hard drawn) at different Temperatures between 12·9° *C. and* 90·2° *C.*—A hard-drawn wire of the A deposit was measured by the apparatus described. For determining

the diameter a piece 300 cm. long was taken. The weight was found to be—

5·6009 grams.

The ascertained density of the copper being 8·959 indicates an average diameter of—

$$(3.)\qquad X = 2\sqrt{\frac{5\cdot6009}{300\times8\cdot959\pi}} = 0\cdot05151 \text{ cm. at } 15^\circ \text{ C.}$$

This same wire was fitted with shunting terminals 250 cm. apart, and wound on one of the previously described reels, and then placed in the circuit. A large number of observations were made at different temperatures: they are arranged in Table II. The following are the headings of the columns :—T_X = temperature of the unknown resistance; G_X = deflection (proportional to the current) with the unknown resistance; G_R = deflection when standard ohm in circuit; T_R = temperature of standard ohm; G_X = deflection of unknown resistance (supposing G_R to be always 1240); G_X (6th column) is deflection with unknown resistance (X) in circuit corrected, so as to compare with G_R at 1240 and T_R at 15° C.; the figures in this column multiplied by a constant give the resistance of the wire.

Table II.

T_X.	G_X.	G_R.	G_X (G_R = 1240).	T_R.	G_X (G_R = 1240, T_R = 15° C.)
C°.				C°.	
12·9	1006	1250	998	13·5	997
16·0	1017	1249	1010	13·7	1010
17·2	1018·5	1245	1014	13·9	1014
18·15	1023·5	1246	1019	,,	1019
19·6	1027	1245	1022	,,	1022
20·3	1030·5	1244·5	1027	,,	1027
20·9	1033	1244	1030	13·8	1030
22·6	1039	1244	1036	,,	1036
23·3	1043	1244·5	1039	11·5	1038
24·2	1045·5	1243	1043	,,	1042
25·2	1048·5	1242	1047	,,	1046
26·2	1052	1242	1050	,,	1049
27·3	1056	1241	1055	,,	1054
28·4	1058·5	1239·5	1059	12·5	1058
29·4	1060	1238·5	1061	,,	1060
30·2	1065	1240	1065	,,	1064
31·1	1069·5	1239·5	1070	12·1	1069
32·2	1073·5	1239	1074	,,	1073
33·2	1077	1238·5	1078	,,	1077
34·2	1080	1237·5	1082	12·5	1081
35·3	1083	1237	1086	,,	1085
36·2	1086·5	1236	1090	,,	1089
37·2	1090·5	1237	1093	,,	1092

Table II—*continued.*

T_X.	G_X.	G_R.	G_X ($G_R = 1240$).	T_R.	G_X ($G_R = 1240$, $T_R = 15°$ C).
C°.				C°.	
38·2	1093	1236	1097	12·5	1096
39·2	1097	1236	1101	"	1100
40·1	1101	1237	1104	"	1103
41·2	1106	1237	1109	"	1108
42·2	1108	1235	1112·5	"	1111
43·5	1111	1234	1116·5	"	1115
44·2	1114·5	1234	1120	"	1119
45·2	1119·5	1235	1124	"	1124
46·2	1123	"	1128	"	1127
47·2	1128	"	1133	"	1132
48·2	1133	1236	1137	"	1136
50·2	1140·5	"	1144	12·8	1143
51·2	936·5	1013	1146	14·5	1146
52	939·2	"	1150	"	1150
53·1	943	"	1154	"	1154
54·1	945·2	1012·7	1157·5	"	1157·5
55·2	949	1013	1162	"	1162
56·2	951·5	1012·5	1165	15·2	1165
57·2	955	1012·5	1169·6	"	1169·6
58·2	958	1013	1173	"	1173
59·2	960·5	1011·5	1177	"	1177
60·2	963	1011	1181	"	1181
61·2	966·5	1011·5	1185	15·5	1185
62·2	969·2	1011	1189	"	1189
63·2	973	"	1193	"	1193
64·2	976	"	1197	"	1197
65·2	979	"	1200	"	1200
66·2	982·5	1012	1204	"	1204
67·2	985·2	1011	1207·4	"	1207·4
68·2	988·5	1011	1212·4	"	1212·4
69·2	991	"	1215·5	"	1215·5
70·2	994	"	1219	"	1219
71·2	997·5	1010	1225	"	1225
72·2	1000	"	1228	"	1228
73·2	1003	"	1231·5	"	1231·5
74·2	1006	"	1235	"	1235
75·2	1010·5	"	1240·5	"	1240·5
76·2	1013	"	1244	"	1244
77·2	1017	"	1249	"	1249
78·2	1020	"	1252	"	1252
79·2	1023·2	"	1256	"	1256
80·1	1027	1011	1260	"	1260
81·2	1031	1010·5	1265	"	1265
82·2	1034	1010·5	1269	"	1269
83·2	1037	1011	1272	"	1272
84·2	1039·5	1010	1276	"	1276
85·2	1043	1010	1280·5	"	1280·5
86·2	1047	1010	1285	"	1285
87·2	1050	1009·5	1290	"	1290
88·2	1052	1009	1293	"	1293
89·2	1056	1009	1298	"	1298
90·2	1060	1009	1303	"	1303

The absolute specific resistance in C.G.S. units was calculated from the above numbers as follows. The resistance in ohms of the measured wire at any temperature is found by using Equation No. 2:—

$$(4.)\qquad X = \frac{G_X(R_X+S)}{G_R(R_R+S)}\,R.$$

From the definition of the R, and that of specific resistance in C.G.S. units, formula 5 is deduced:—

$$(5.)\qquad \sigma = X\frac{\pi r^2}{l}\,10^9,$$

where r = radius of the wire in centimetres.
l = length " " "

If the value of X as given in Equation No. 2 is substituted in Equation No. 5, the following is obtained:—

$$(6.)\qquad \sigma = \frac{G_X(R_X+S)}{(R_R+S)}\,\frac{R}{G_R}\,\frac{\pi r^2}{l}\,10^9.$$

In the previous table if G_X *is read as in the last column*, the following will be the values for the various elements of Equation No. 6:—

R_R = 8000 B.A. units for all observations.
R_X = 2000 " " "
S = 17·3 " " "
R = 1 ohm.
G_R = 1240. Scale divisions for all observations.
$2r$ = 0·05151 cm.
l = 250· "
At 15° C. But, according to Matthiessen, no correction is made for the variation of these constants.

In Equation No. 6 everything is constant except σ and G_X, and the equation results in the following, when the numerical values as above are substituted for the general expressions:—

$$(7.)\qquad \sigma = 1{\cdot}6906\; G_X\begin{pmatrix}G_R = 1240\\ T_R = 15^\circ\ \mathrm{C.}\end{pmatrix}$$

$$\log 1{\cdot}6906 = 0{\cdot}2280258.$$

The specific resistance in C.G.S. units of the hard-drawn rocking tank A deposit at any temperature can therefore be calculated by making use of the values given in the above table. In order to find the specific resistance at 0° C. it is necessary to calculate the temperature coefficient (Δ_t) from the above observations. The method of

least squares is the one which naturally suggests itself in calculating this coefficient, but the calculation would be too laborious in comparison with the value of the figure arrived at. Therefore, in order to ascertain which values could be used for calculating the temperature coefficient (Δ_t), the above observations were plotted in the usual way, the co-ordinates being x = temperature and y = specific resistance in C.G.S. units.

As previously stated, the values in column 6 of Table II have to be multiplied by 1·6906 to give the specific resistance in C.G.S. units; to save labour the multiplication was done graphically (Graphic Table No. 1).

The resistances in column 6 of Table I reduced to C.G.S. units by means of the graphical Table 1 are tabulated in another table (Table III).

The graphic Table No. 2 is a reproduction of Table III, and gives the results of the above measurements.

As the values formed so very nearly a straight line, two only are selected :—

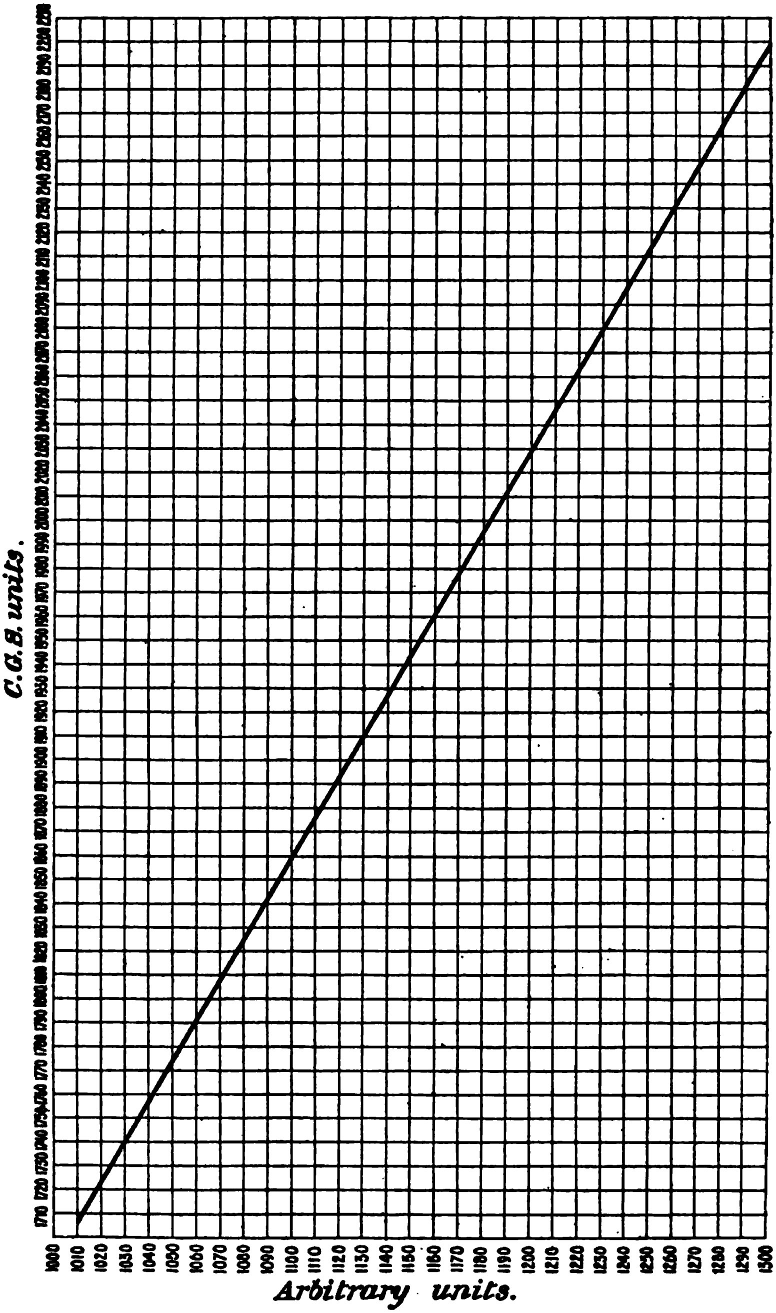
1707 C.G.S. units at 16·0° C.
1937 „ „ 51·2° C.

These gave the following equations :—

$$1707 = x(1+16y)$$
$$1937 = x(1+51{\cdot}2y), \text{ from which}$$
$$x = 1603{\cdot} \text{ C.G.S. units}$$
$$y = 0{\cdot}004077.$$

x is the specific resistance of the A deposit (hard drawn) at 0° C., and y is the temperature coefficient. By means of these values an extrapolation was made from 0° to 16° C. in the graphic Table No. 2.

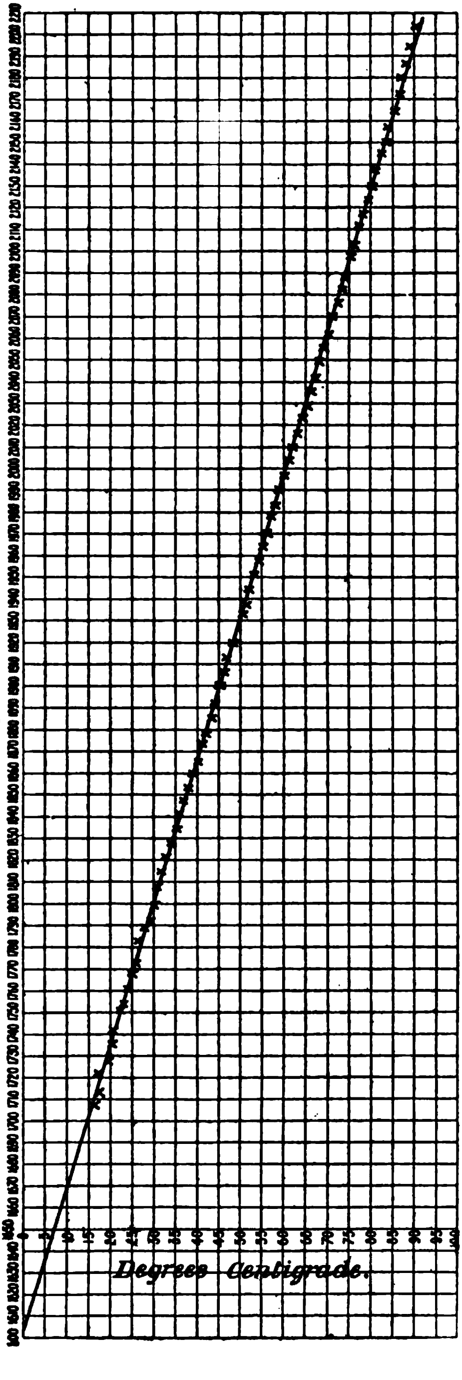
Graphic Table 1.



Reduction of column 6, Table II, from arbitrary units.
To C.G.S. units by graphic method (multiplication by 1·6906).

Graphic Table 2.

Spec. resistance of Rockingtank A. copper in C.G.S. units.



Shows the result of the measurements of the specific resistance of Rocking tank A copper (hard-drawn). The crosses indicate the actual observations reduced to the C.G.S. system.

Table III.

Rocking Tank A Copper. Hard drawn.

Specific Resistance.

Temperature.	C.G.S. units.	Temperature.	C.G.S. units.	Temperature.	C.G.S. units.
C°.		C°.		C°.	
16·0	1707	47·2	1913	79·2	2123
17·2	1713	48·2	1920	80·1	2130
18·1	1722	50·2	1932	81·2	2138
19·6	1727	51·2	1937	82·2	2145
20·3	1735	52·0	1944	83·2	2150
20·9	1741	53·1	1951	84·2	2157
22·6	1751	54·1	1957	85·2	2164
23·3	1754	55·2	1964	86·2	2172
24·2	1761	56·2	1970	87·2	2180
25·2	1768	57·2	1978	88·2	2186
26·2	1773	58·2	1983	89·2	2194
27·3	1783	59·2	1990	90·2	2203
28·7	1787	60·2	1996		
29·7	1792	61·2	2003		
30·2	1799	62·2	2010		
31·1	1807	63·2	2016		
32·2	1814	64·2	2023		
33·2	1821	65·2	2029		
34·2	1827	66·2	2035		
35·3	1834	67·2	2041		
36·2	1841	68·2	2049		
37·2	1847	69·2	2055		
38·2	1853	70·2	2061		
39·2	1860	71·2	2070		
40·1	1865	72·2	2076		
41·2	1873	73·2	2082		
42·2	1878	74·2	2088		
43·5	1885	75·2	2097		
44·2	1892	76·2	2103		
45·2	1900	77·2	2111		
46·2	1906	78·2	2117		

Measurements with Rocking Tank A Copper, Soft.

A similar wire to that used in the previous measurements was annealed in a tube of hard glass, through which was passing a current of dry CO_2 gas, in order to prevent oxidation. The following measurements were made with it:—

Density at 15° C., 8·959 (see previous table).
Absolute weight of 300 cm. of the wire, 5·5746 grams.
Diameter.

$$X = 2r = 2\sqrt{\frac{5\cdot5746}{\pi\ 8\cdot959\times300}} = 2\times0\cdot025694 = 0\cdot051388 \text{ cm.}$$

250 cm. of this wire were compared with the standard ohm, according to the method described. The following are the observations:—

Table IV.

T_X.	G_X.	G_R.	G_X ($G_R = 1240$).	T_R.	G_X ($G_R = 124°$ $T_R = 15°C.$).
16·8° C.	820·	1023	994	20° C.	—
,,	819·5	1023	993	,,	994
19·95	829·	1020	1008	,,	—
,,	828·5	1020	1007	,,	1008
48·0	916·	1019	1114	,,	—
,,	916·	1019	1114	,,	1115

From the numbers in column 6 the values of the specific resistance of this sample can be obtained by multiplication with the constant 1·6826 obtained in the same manner as the value for the hard variety, the difference being due to the difference in the diameters. All the other constants of the measurements were the same. The results by calculation give the following values:—

Specific resistance of rocking tank A copper, annealed in CO_2 gas:—

At 16·8° C. = 1672·4 C.G.S. units.
,, 19·95 = 1696·0 ,, ,,
,, 48·0 = 1876·0 ,, ,,

The temperature coefficient, calculated from the following formula:—

(8.) $$R_t = R_0(1+xt),$$

gives the value 0·00418 at ordinary temperatures.

Applying this value of x to the amount of specific resistance at 16·8° C., the following is obtained as the specific resistance of this special sample of annealed rocking tank A deposit:—

$$\sigma_{0°C} = 1566 \text{ C.G.S. units.}$$

Specific Resistance of Soft Annealed Copper Wire made from Deposited Sheet Copper named B, the Deposit being obtained from a Solution of Rocking Tank A Copper Dissolved in Pure Diluted Sulphuric Acid, using an Anode of the Tank A Copper.

Origin of the Copper.—A solution of sulphate of copper was prepared by using a piece of rocking tank A copper, as the anode in a

mixture of pure sulphuric acid and distilled water, the cathode being another strip of copper enclosed in a new and clean cell of porous earthenware. The current was continued until the solution had the desired strength. From this solution a sheet of copper was deposited on a polished copper plate, as described in the first part of this paper, the rocking tank A copper still being the anode. The deposit was then cut into narrow strips and drawn through sapphire dies to the requisite diameter. It was finally annealed by heating in a current of CO_2 gas.

Specific Gravity of the Copper B.—It was ascertained by weighing equal lengths of this wire and sample A (both drawn through the same die), that their specific gravities did not differ to any appreciable amount, the density 8·959 at 15° C. was therefore taken for this sample B.

Diameter of the Wire.—300 cm. of the wire weighed 5·5845 grams; the diameter is therefore

$$X = 2\sqrt{\frac{5\cdot5845}{\pi 8\cdot959 \times 300}} = 2 \times 0\cdot025717 = 0\cdot051434 \text{ cm.}$$

Electrical Measurements.—The same arrangements were employed as previously.

Table V.

T_X.	G_X.	G_R.	G_X ($G_R = 1240$).	T_R.	G_X ($G_R = 1240$, $T = 15°$C.).
15·9° C.	832	1046·5	985·4	17·5° C.	986
,,	831	1046		,,	
,,	829	1044		,,	
,,	830	1045		,,	

To ascertain the specific resistance in C.G.S. units, the constant by which to multiply 986 (last column, Table V) was calculated in the same manner as in Equation No. 7, and by substituting 0·025717 for r instead of 0·025755, the constant 1·6856 is obtained. We thus have the specific resistance of the pure copper :—

$$\sigma_{15\cdot9°\text{C.}} = 986 \times 1\cdot6856 = 1662 \text{ C.G.S. units.}$$

Measurements at higher temperatures for ascertaining the temperature coefficient were made, but the data were unfortunately lost. The result, however, was

$$\Delta_t = 0\cdot00415.$$

Applying this constant in the well-known formula, the following is found to be the specific resistance at 0° C.:—

$$\sigma_{0^\circ C.}\ 1559{\cdot}1 \text{ C.G.S. units.}$$

Table of General Results.

		C.G.S. units.		Temp. coefficient Δt.
Specific resistance of A deposit (hard)....................	At 0° C. =	1603	....	0·00408
Same wire after annealing in CO_2	At 0° C. =	1566	....	0·00418
Specific resistance of Sample B (annealed)................	At 0° C. =	1559	....	0·00415

As the difference between the last two values only amounts to 0·4 per cent., it is probable that both of the specimens were perfectly pure, and that the limit of electrolytic purification had been reached. The mean of the two gives the probable specific resistance of pure copper. Thus, as a general conclusion, it may be stated that the specific resistance for pure copper (hard and annealed) is:—

Hard variety, wire 1603 C.G.S. units and $\Delta_t = 0{\cdot}00408$.
Soft „ „ 1563 „ „ $= 0{\cdot}00416$.

Presents, May 24, 1894.

Transactions.

Baltimore:—Johns Hopkins University. Circulars. Vol. XIII. No. 111. 4to. *Baltimore* 1894; Studies in Historical and Political Science. Series XII. No. 4. 8vo. *Baltimore* 1894. The University.

Breslau:—Schlesische Gesellschaft für Vaterländische Cultur. Jahres-Bericht. Bd. LXX. 8vo. *Breslau* 1893. The Society.

Calcutta:—Indian Museum. Notes. Vol. II. No. 7. 8vo. *Calcutta* 1893. The Museum.

Dublin:—Royal Dublin Society. Scientific Transactions. Vol. IV. Part 14. Vol. V. Parts 1—4. 4to. *Dublin* 1892–93; Scientific Proceedings. Vol. VII. Part 5. Vol. VIII. Parts 1—2. 8vo. *Dublin* 1892–93. The Society.

Frankfort-on-the-Main:—Senckenbergische Naturforschende Gesellschaft. Abhandlungen. Bd. XVIII. Heft 2. 4to. *Frankfurt a. M.* 1894. The Society.

Göttingen:—Königl. Gesellschaft der Wissenschaften. Nachrichten. 1894. No. 1. 8vo. *Göttingen.* The Society.

Transactions (*continued*).

Halle :—Kais. Leopoldinisch - Carolinische Deutsche Akademie der Naturforscher. Verhandlungen. Bd. LIX—LX. 4to. *Halle* 1893–94; Leopoldina. Heft 29. 4to. *Halle* 1893.
The Academy.

Kew :—Royal Gardens. Bulletin of Miscellaneous Information. 1894. No. 89. 8vo. *London.* The Director.

Kiel:—Königl. Universität. Schriften. 1892–93. 8vo.
The University.

La Plata :—Museo. Anales. Paleontologia Argentina. Tomo II. Folio. *La Plata* 1893; Revista. Tomo IV. 8vo. *La Plata* 1893. The Museum.

Leipsic :—Königl. Sächs. Gesellschaft der Wissenschaften. Abhandlungen (Phil.-hist. Classe). Bd. XIV. No. 5. 8vo. *Leipzig* 1894. The Society.

London :—Odontological Society. Transactions. Vol. XXVI. No. 6. 8vo. *London* 1894. The Society.

University. Calendar. 1894–95. 8vo. *London* 1894.
The University.

Zoological Society. Report of the Council. 1893. 8vo. *London* 1894. The Society.

Manchester :—Literary and Philosophical Society. Memoirs and Proceedings. Vol. VIII. No. 2. 8vo. *Manchester* 1894.
The Society.

Munich :—K. B. Akademie der Wissenschaften. Abhandlungen (Math.-phys. Classe). Bd. XVIII. Abth. 1—2. 4to. *München* 1893; Gedächtnisrede auf Karl von Nägeli. 4to. *München* 1893; Ueber die Wege und Ziele der Hirnforschung: Festrede von N. Rüdinger. 4to. *München* 1893.
The Academy.

New York :—American Geographical Society. Bulletin. Vol. XXV. No. 4. Part 2. Vol. XXVI. No. 1. 8vo. *New York* 1893–94. The Society.

Paris :—Société Mathématique. Bulletin. Tome XXII. No. 3. 8vo. *Paris* [1894]. The Society.

Penzance :—Royal Geological Society of Cornwall. Transactions. Vol. XI. Parts 6—8. 8vo. *Penzance* 1892–94. The Society.

Santiago :—Société Scientifique du Chili. Actes. Tome III. Livr. 3. 8vo. *Santiago* 1894. The Society.

Siena :—R. Accademia dei Fisiocritici. Atti. Vol. VI. Fasc. 4—5. 8vo. *Siena* 1894; Processi Verbali delle Adunanze. No. 3. 8vo. *Siena* 1894. The Academy.

Toronto :—Canadian Institute. Transactions. Vol. IV. Part 1. 8vo. *Toronto* 1894; Seventh Annual Report. 8vo. *Toronto* 1894. The Institute.

Transactions (*continued*).

Turin:—R. Università degli Studi. Annuario. 1893–94. 8vo. *Torino* 1894. The University.

Wellington, N.Z.:—Polynesian Society. Journal. Vol. III. No. 1. 8vo. *Wellington, N.Z.* 1894. The Society.

Observations and Reports.

Calcutta:—Meteorological Department, Government of India. Indian Meteorological Memoirs. Vol. VI. Part 1. 4to. *Calcutta* 1894. The Department.

Greenland:—Commissionen for Ledelsen af de Geologiske og Geographiske Undersøgelser. Meddelelser om Grønland. Hefte 3. Fortsættelse 1—4. Hefte 7—13. 8vo. *Kjøbenhavn* 1887–94. The Commission.

Pulkowa:—Observatoire Central Nicolas. Observations de Poulkovo. Vol. X. Folio. *St. Pétersbourg* 1893; Publications. Série 2. Vol. I. Folio. *St. Pétersbourg* 1893; Russische Expeditionen zur Beobachtung des Venusdurchgangs, 1874. Abth. 1. 4to. *St. Petersburg* 1891; Tables Auxiliaires pour la Détermination de l'Heure par des Hauteurs Correspondantes de Différentes Étoiles. Construites par Th. Wittram. 8vo. *St. Pétersbourg* 1892. The Observatory.

Sydney:—Observatory. Meteorological Observations. November—December, 1893. 8vo. [*Sydney.*] The Observatory.

Washington:—U.S. Department of Agriculture. Monthly Weather Review. February, 1894. 4to. *Washington;* Report of the Ohio Weather and Crop Service. March, 1894. 8vo. *Norwalk, Ohio.* The Department.

Journals.

Acta Mathematica. Bände XI—XVIII, Heft. 1. 4to. *Stockholm* 1888–94. The Editor.

Annaes de Sciencias Naturaes. Anno 1. No. 2. 8vo. *Porto* 1894. The Editor.

Astronomy and Astro-Physics. May, 1894. 8vo. *Northfield, Minn.* The Editor.

Epigraphia Indica of the Archæological Survey of India. Vol. II. Part 14. Folio. *Calcutta* 1893. Revenue and Agricultural Department, Calcutta.

Journal of Comparative Neurology. April, 1894. 8vo. *Granville, Ohio.* The Editors.

Stazioni Sperimentali Agrarie Italiane. Vol. XXVI. Fasc. 3. 8vo. *Modena* 1894. R. Stazione Agraria, Modena.

Journals (*continued*).

Zeitschrift für Naturwissenschaften. Bd. LXVI. Heft 5—6. 8vo. *Leipzig* 1894.

Naturwissenschaftlicher Verein, Halle.

Döllen (W.) Stern-Ephemeriden auf das Jahr 1894. 8vo. *Dorpat* 1894. The Author.

Girouard (D.) Lake St. Louis Old and New Illustrated and Cavelier de La Salle. 8vo. *Montreal* 1893. The Author.

Hunt (A. R.) On certain Affinities between the Devonian Rocks of South Devon and the Metamorphic Schists. 8vo. *London* 1892; [and three other pamphlets. 8vo.] The Author.

Hutchinson (Rev. H. N.) Creatures of other Days. 8vo. *London* 1894. The Publishers.

Marsh (O. C.) Restoration of *Elotherium*. 8vo. [*New Haven*] 1894. The Author.

May 31, 1894.

The LORD KELVIN, D.C.L., LL.D., President, followed by Sir JOHN EVANS, K.C.B., D.C.L., LL.D., Vice-President and Treasurer, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the Electrification of Air." By LORD KELVIN, P.R.S., and MAGNUS MACLEAN, M.A., F.R.S.E. Received May 9, 1894.

§ 1. That air can be electrified either positively or negatively is obvious from the fact that an isolated spherule of pure water, electrified either positively or negatively, can be wholly evaporated in air.* Thirty-four years ago it was pointed out by one of

* This demonstrates an affirmative answer to the question, Can a molecule of a gas be charged with electricity? (J. J. Thomson, 'Recent Researches in Electricity and Magnetism,' § 36, p. 53) and shows that the experiments referred to as pointing to the opposite conclusion are to be explained otherwise.

Since this was written, we find, in the 'Electrical Review' of May 18, on page 571, in a lecture by Elihu Thomson, the following:—"It is known that as we leave the surface of the earth and rise in the air, there is an increase of positive potential

us* as probable that in ordinary natural atmospheric conditions, the air for some considerable height above the earth's surface is electrified,† and that the incessant variations of electrostatic force which he had observed, minute after minute, during calms and light winds, and often under a cloudless sky, were due to motions of large quantities of positively or negatively electrified air in the immediate neighbourhood of the place of observation.

§ 2. It was proved‡ by observations in the Old College of Glasgow University that the air was in general negatively electrified, not only indoors, within the old lecture room§ of Natural Philosophy, but also in the out-of-doors space of the College Court, open to the sky though closed around with high buildings, and between it and the top of the College Tower. The Old College was in a somewhat low situation, surrounded by a densely crowded part of a great city. In the new University buildings, crowning a hill on the western boundary of Glasgow, similar phenomena, though with less general

with respect to the ground. . . . . It is not clearly proven that a pure gas, rarefied or not, can receive and convey a charge. If we imagine a charged drop of water suspended in air and evaporating, it follows that, unless the charge be carried off in the vapour, the potential of the drop would rise steadily as its surface diminished, and would become infinite as the drop disappeared, unless the charge were dissipated before the complete drying up of the drop by dispersion of the drop itself, or conveyance of electricity by its vapour. The charge would certainly require to pass somewhere, and might leave the air and vapour charged."

It is quite clear that "must" ought to be substituted for "might" in this last line. Thus the vagueness and doubts expressed in the first part of the quoted statement are annulled by the last three sentences of it.

* "Even in fair weather the intensity of the electric force in the air near the earth's surface is perpetually fluctuating. The speaker had often observed it, especially during calms or very light breezes from the east, varying from 40 Daniell's elements per foot to three or four times that amount during a few minutes, and returning again as rapidly to the lower amount. More frequently he had observed variations from about 30 to about 40, and back again, recurring in uncertain periods of perhaps about two minutes. These gradual variations cannot but be produced by electrified masses of air or cloud, floating by the locality of observation."—Lord Kelvin's 'Electrostatics and Magnetism,' art. xvi, § 282.

† The out-of-doors air potential, as tested by a portable electrometer in an open place, or even by a water-dropping nozzle outside, two or three feet from the walls of the lecture room, was generally on these occasions positive, and the earth's surface itself therefore, of course, negative—the common fair weather condition—which I am forced to conclude is due to a paramount influence of positive electricity in higher regions of the air, notwithstanding the negative electricity of the air in the lower stratum near the earth's surface. On the two or three occasions when the in-door atmospheric electricity was found positive, and, therefore, the surface of the floor, walls and ceiling negative, the potential outside was certainly positive, and the earth's surface out-of-doors negative, as usual in fine weather."—*Ibid.*, § 300.

‡ *Ibid.*, Q. 2, § 283.

§ *Ibid.*, §§ 296—300.

prevalence of negative electricity in the air, have been observed, both indoors, in the large Bute Hall, and in many other smaller rooms, and out-of-doors, in the court, which is somewhat similar to the courts of the Old College, but much larger. It is possible that the negative electricity found thirty years ago in the air of the Old College, may have been due to its situation, surrounded by houses with their fires, and smoking factory chimneys. In the New College much of the prevalence of negative electricity in air within doors has, however, been found to be due to electrification by the burning lamp* used with the quadrant electrometer; and more recent observations, with electrification by flame absolutely excluded, throw doubt on the old conclusion, that both in town and country negative electrification is the prevailing condition of natural atmospheric air in the lower regions of the atmosphere.

§ 3. The electric ventilation found in the Old College, and described in § 299 of "Electrostatics and Magnetism," according to which air drawn through a chink, less than $\frac{1}{2}$ in. wide, of a slightly open window or door, into a large room, showed the electrification which it had on the other side of the chink, whether that was the natural electrification of the open air, or positive or negative electrification produced by aid of a spirit lamp and electric machine in an adjoining room, has been tried again in the new College with quite corresponding results. It has also been extended to the drawing in of electrified air through a tube to the enclosure represented in fig. 1 of the present paper; with the result that the water-dropping test indicated in the sketch, amply sufficed to show the electrification, and verify that it was always the same as that of the air outside. When the tube was filled with loosely packed cotton-wool the electrification of the entering air was so nearly annulled as to be insensible to the test.

§ 4. The object proposed for the experiments described in the present communication was to find if a small unchanged portion of air could be electrified sufficiently to show its electrification by ordinary tests, and could keep its electrification for any considerable time; and to test whether or not dust in the air is essential to whatever of electrification might be observed in such circumstances, or is much concerned in it.

§ 5. The arrangement for the experiments is shown in the diagram, Fig. 1. AA is a large sheet-iron vat inverted on a large wooden tray BB, lined with lead. By filling the tray with water the air is confined in the vat. There are two holes in the top of the vat:

* 'Electrification of Air by Combustion.' Magnus Maclean, M.A., F.R.S.E., and Makita Goto, Philosophical Society of Glasgow, November 20, 1889; 'Electrification of Air by Water Jet,' Magnus Maclean, M.A., F.R.S.E., and Makita Goto, 'Philosophical Magazine,' August, 1890.

Fig. 1.

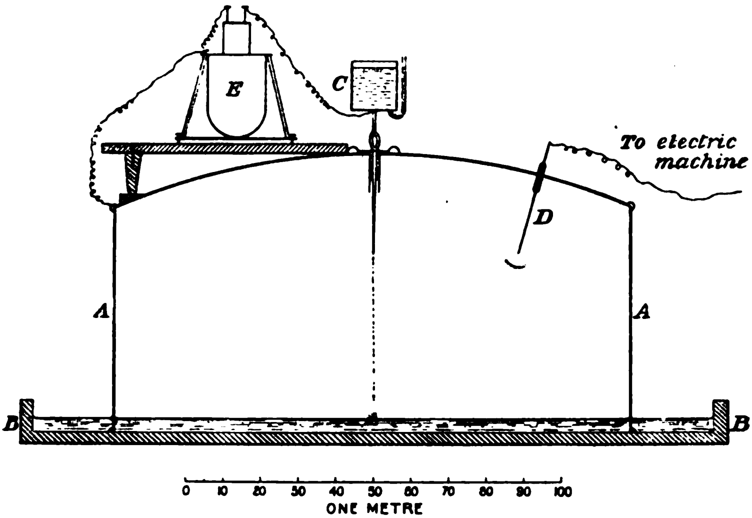
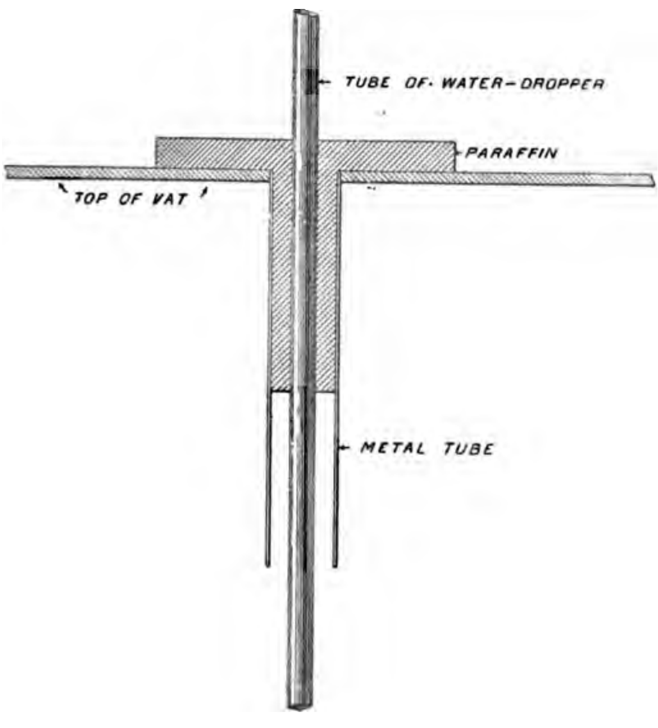

E
C
To electric machine
D
A
A
B
B
0 10 20 30 40 50 60 70 80 90 100
ONE METRE

Fig. 2.


TUBE OF WATER-DROPPER
PARAFFIN
TOP OF VAT
METAL TUBE

one for the water-dropper C, and one for the charging wire D. Both the water-dropper, and the charging wire, ending with a pin-point as sharp as possible, are insulated by solid paraffin, which is surrounded by a metal tube, as shown in half size in Fig. 2. To start with they were supported by pieces of vulcanite embedded in paraffin. But it was found that after the lapse of some days, (possibly on account of ozone generated by the incessant brush discharges), the insulation had utterly failed in both of them. The vulcanite pieces were then taken out, and solid paraffin, with the metal guard-tube round it to screen it from electrically influencing the water-dropper, was substituted. This has proved quite satisfactory: the water-dropper, with the flow of water stopped, holds a positive or a negative charge for hours.

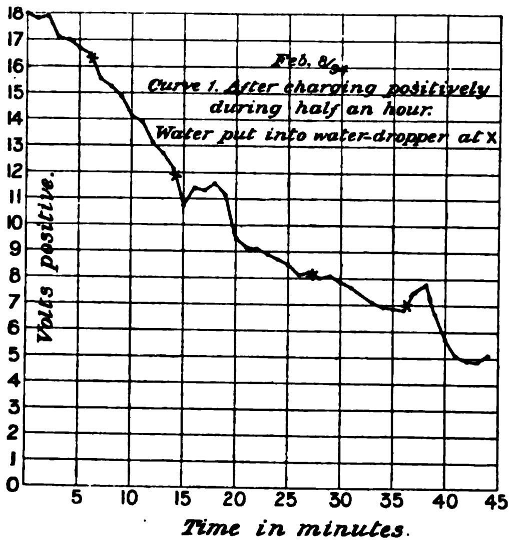
§ 6. A quadrant electrometer E (described in "Electrostatics and Magnetism," §§ 346—353) was set up on the top of the vat near the water-dropper, as shown in Fig. 1. It was used with lamp and semi-transparent scale to indicate the difference of potential between the water-dropper and the vat. The sensibility of the electrometer was 21 scale divisions (half-millimetres) per volt, and as the scale was 90 centimetres long, difference of potentials up to 43 volts positive or negative, could be read by adjusting the metallic zero to the middle of the scale. A frictional plate-electric machine was used, and by means of it, in connection with the pin-point, the air inside the vat could be electrified either positively or negatively.

§ 7. The vat was fixed in position in the Apparatus Room of the Natural Philosophy Department of the University of Glasgow on the 13th of December, 1893, and for more than three months the air inside was left undisturbed except by discharges from the pin-point through the electrifying wire, and by the spray from the water-dropper. Thus the air was becoming more and more freed of dust day by day. Yet at the end of the four months we found that the air was as easily electrified, either positively or negatively, as it was at the beginning; and that if we electrify it strongly by turning the machine for half-an-hour, it retains a considerable portion of this electrification for several hours.

§ 8. Observations were taken almost daily since the 13th December; but the following, taken on the 8th of February, the 12th of March, and the 23rd of April, will serve as specimens, the results being shown in each case by a curve. At all these dates the air must have been very free from dust. Both during the charging and during the observations the case of the electrometer and one pair of quadrants are kept metallically connected to the vat. During the charging the water-dropper and the other pair of quadrants were also kept in connection with the vat. Immediately after the charging was stopped the charging-wire was connected metallically to the out-

side of the vat, and left so with its sharp point unchanged in its position inside the vat during all the observations.

§ 9. *Curve* 1. *February* 8, 1894.—The friction-plate machine was turned positive for half-an-hour. Ten minutes after the machine stopped the water-dropper was filled and joined to one pair of quadrants of the electrometer, while the other pair was joined to the case of the instrument. The first reading on the curve was taken four minutes afterwards, that is fourteen minutes after the machine stopped running (18 volts.).



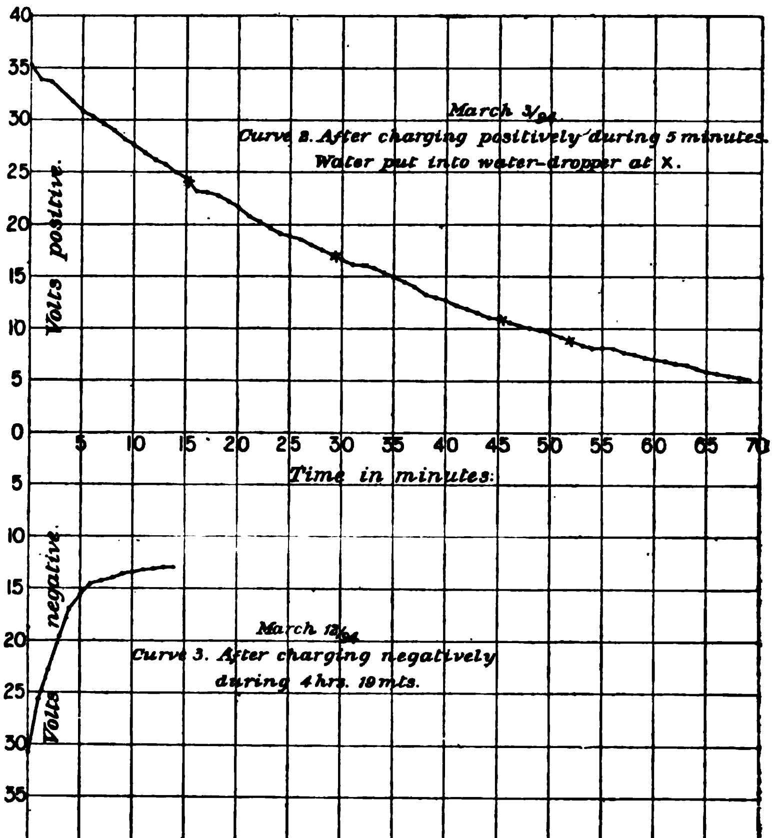
Curve 2. *March* 3, 1894.—The friction-plate machine was turned positive for five minutes. The water-dropper was filled and joined to the electrometer immediately after the machine stopped turning. The spot was off the scale, and nine minutes elapsed before it appeared on the scale. The first reading on the curve was taken one minute afterwards, or ten minutes after the machine stopped turning (35·25 volts).

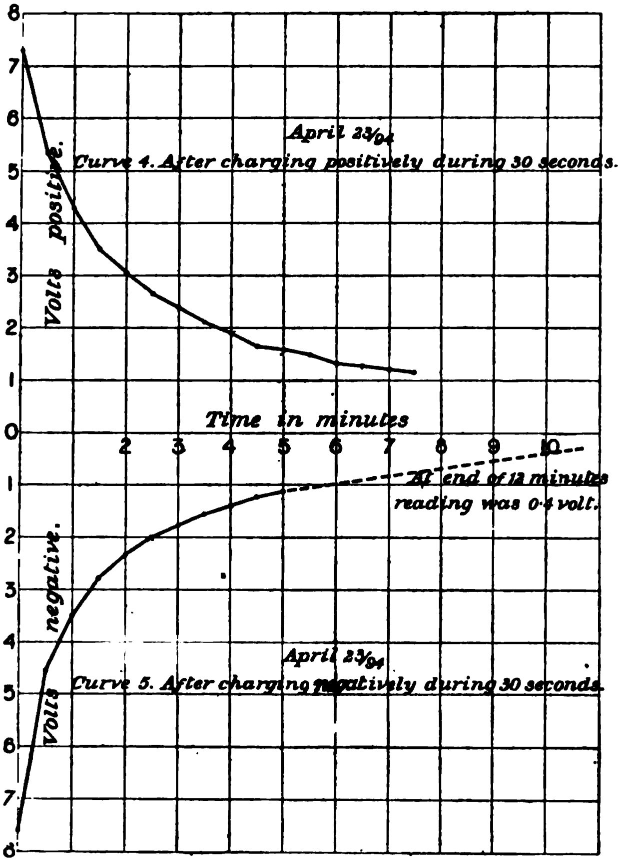
Curve 3. *March* 12, 1894.—A Voss induction machine was joined to the charging wire, and run by an electric motor for 4 hours 19 minutes. A test was applied at the beginning of the run to make sure that it was charging negatively; and a similar test when it was disconnected from the charging wire in the vat showed it to be still charging negatively. The water-dropper was joined to the electrometer, and the spot appeared on the scale immediately. The first reading on the curve was taken half a minute after the machine was disconnected (30·65 volts).

Curve 4. *April* 23, 1894.—The friction-plate machine was turned positive for 30 seconds, with water-dropper running and joined to the electrometer. 20 seconds after the machine stopped the spot appeared on the scale, and the reading 1½ minutes after the machine stopped turning is the first point on the curve (7·3 volts).

Curve 5. *April* 23, 1894.—The friction-plate machine was turned negative for 30 seconds, with the water-dropper running and joined to the electrometer. 10 seconds afterwards the spot appeared on the scale, and the reading 70 seconds after the machine stopped turning is the first point on the curve (7·6 volts).

The curves show, what we always found, that the air does not retain a negative electrification so long as it retains a positive. We also found, by giving equal numbers of turns to the machine that the immediately resulting difference of potential between the water-dropper and the vat was greater for the negative than for the posi-





tive electrification; though the quantity received from the machine was probably less in the case of the negative electrification, because the negative conductor was less well insulated than the positive.

§ 10. On the 21st of March, two U-tubes were put in below the edge of the vat, one on either side, so that it might be possible to blow dusty, or smoky, or dustless air into the vat. To one tube was fitted a blowpipe bellows, and by placing it on the top of a box in which brown paper and rosin were burning, the vat was filled with smoky air. Again, several layers of cotton-wool were placed on the mouth of the bellows, so as to get dustless air into the vat. The bellows were worked for several hours on four successive days, and we found no appreciable difference (1) in the ease with which the air could be electrified by discharges from the wire connected to the electric machine, and (2) in the length of time the air retains its electrification.

But it was found that, as had been observed four years ago with the same apparatus,* with the water-dropper insulated and connected to the electrometer, and no electrification of any kind to begin with, a negative electrification amounting to four, five, or six volts gradually supervened if the water-dropper was kept running for 60 or 70 minutes, through air which was dusty, or natural, to begin with. It was also found, as in the observations of four years ago, that no electrification of this kind was produced by the dropping of the water through air purified of dust.

The circular bend of the tube of the water-dropper shown in the drawing was made for the purpose of acting as a trap to prevent the natural dusty air of the locality from entering the vat when the water-dropper ran empty.

§ 11. The equilibrium of electrified air within a space enclosed by a fixed bounding surface of conducting material presents an interesting illustration of elementary hydrostatic principles. The condition to be fulfilled is simply that the surfaces of equal electric "volume-density" are surfaces of equal potential, if we assume that the material density of the air at given temperature and pressure is not altered by electrification. This assumption we temporarily make from want of knowledge; but it is quite possible that experiment may prove that it is not accurately true; and it is to be hoped that experimental investigation will be made for answering this very interesting question.

§ 12. For stable equilibrium it is further necessary that the electric density, if not uniform throughout, diminishes from the bounding surface inwards. Hence, if there is a portion of non-electrified air in the enclosure it must be wholly surrounded by electrified air.

§ 13. We may form some idea of the absolute value of the electric density, and of the electrostatic force in different parts of the enclosure, in the electrifications found in our experiments, by considering instead of our vat a spherical enclosure of diameter intermediate between the diameter and depth of the vat which we used. Consider, for example, a spherical space enclosed in metal of 100 cm. diameter, and let the nozzle of the water-dropper be so placed that the stream breaks into drops at the centre of the space. The potential shown by the electrometer connected with it, being the difference between the potentials of the air at the boundary and at the centre, will be the difference of the potentials at the centre due respectively to the total quantity of electricity distributed through the air and the equal and opposite quantity on the inner boundary of the enclosing metal; and we therefore have the formula:—

$$V = 4\pi \int_0^a \rho \left(\frac{r^2}{r} - \frac{r^2}{a}\right) dr,$$

* Maclean and Goto, 'Philosophical Magazine,' August, 1890.

where V denotes the potential indicated by the water-dropper, a the radius of the spherical hollow, and ρ the electric density of the air at distance r from the centre. Supposing now, for example, ρ to be constant from the surface to the centre (which may be nearly the case after long electrification as performed in our experiments), we find $V = \frac{2}{3}\pi\rho a^2$; whence $\rho = 3V/2\pi a^2$.

To particularise further, suppose the potential to have been 38 volts or 0·127 electrostatic c.g.s. (which is less than the greatest found in our experiments) and take $a = 50$ cm.: we find $\rho = 2{\cdot}4.10^{-5}$. The electrostatic force at distance r from the centre, being $\frac{4}{3}\pi\rho r$, is therefore equal to $10^{-4}r$. Hence a small body electrified with a quantity of electricity equal to that possessed by a cubic centimetre of the air, and placed midway ($r = 25$) between the surface and centre of the enclosure experiences a force equal to $2{\cdot}4.10^{-9}.25$, or 6.10^{-8}, or approximately 6.10^{-5} grammes weight. This is 4·8 per cent. of the force of gravity on a cubic centimetre of air of density 1/800.

§ 14. Hence we see that, on the supposition of electric density uniform throughout the spherical enclosure, each cubic centimetre of air experiences an electrostatic force towards the boundary in simple proportion to distance from the centre, and amounting at the boundary to nearly 10 per cent. of the force of gravity upon it; and electric forces of not very dissimilar magnitudes must have acted on the air electrified as it actually was in the non-spherical enclosure used in our experiments. If natural air or cloud, close to the ground or in the lower regions of the earth's atmosphere, is ever, as in all probability it often is, electrified to as great a degree of electric density as we have found it within our experimental vat, the natural electrostatic force in the atmosphere, due as it is, no doubt, to positive electricity in very high regions, must exercise an important ponderomotive force quite comparable in magnitude with that due to difference of temperatures in different positions.

It is interesting to remark that negatively electrified air over negatively electrified ground, and with non-electrified air above it, in an absolute calm, would be in unstable equilibrium; and the negatively electrified air would therefore rise, probably in large masses, through the non-electrified air up to the higher regions, where the positive electrification is supposed to reside. Even with no stronger electrification than that which we have had within our experimental vat, the moving forces would be sufficient to produce instability comparable with that of air warmed by the ground and rising through colder air above.

§ 15. During a thunderstorm the electrification of air, or of air and the watery spherules constituting cloud, need not be enormously stronger than that found in our experiments. This we see by considering that if a uniformly electrified globe of a metre diameter

produces a difference of potential of 38 volts between its surface and centre, a globe of a kilometre diameter, electrified to the same electric density, reckoned according to the total electricity in any small volume (electricity of air and of spherules of water, if there are any in it), would produce a difference of potential of 38 million volts between its surface and centre. In a thunderstorm, flashes of lightning show us differences of potentials of millions of volts, but not perhaps of many times 38 million volts, between places of the atmosphere distant from one another by half a kilometre.

II. "On the Effect of Magnetisation upon the Dimensions of Iron Rings in Directions perpendicular to the Magnetisation, and upon the Volume of the Rings." By SHELFORD BIDWELL, M.A., LL.B., F.R.S. Received March 2, 1894.

A recent communication* to the Society contained an account of some experiments relating to the effects of magnetisation upon the dimensions of two iron rings, one of which was annealed and the other hardened. The rings had the form of short cylinders about 6 cm. in diameter, 3 cm. in height, and 0·4 cm. in thickness. The experiments in question were concerned with the circumferential variations which took place along the lines of magnetisation; those to be here described deal with the concomitant variations in the height of the cylinders (width of the rings) transversely to the magnetisation. On the assumption that variations similar to the latter occur at the same time in the thickness of the metal, it is possible to deduce the changes in the volume of the ring which attend magnetisation.

Fig. 1, from a photograph, shows how the rings were prepared for the experiments. Four brass rods were hard-soldered to the iron, two of them being in a line with a diameter, while the other two were attached to the edges, opposite to one another, and parallel to the axis of the ring. The ring was inserted in a wooden case, also shown, through holes in which the four brass rods projected. Insulated wire for carrying the magnetising current was wound over the wooden jacket.

For the new experiments the ring was placed in a horizontal position, one of the edge rods resting upon a brass socket on the adjustable base of the instrument, and the other, which had a chisel-shaped end, actuating the lever.† To counterbalance the weight of the ring a horizontal arm, carrying a sliding weight, was fixed to the lower rod.

* 'Roy. Soc. Proc.,' vol. 55, p. 228.

† The chisel-shaped terminal piece was removable and is not shown in fig. 1.

Fig. 1.

It need hardly be said that the experimental difficulties in the way of determining to a ten-millionth part the changes which took place in a length of less than $1\frac{1}{4}$ in. were very considerable.

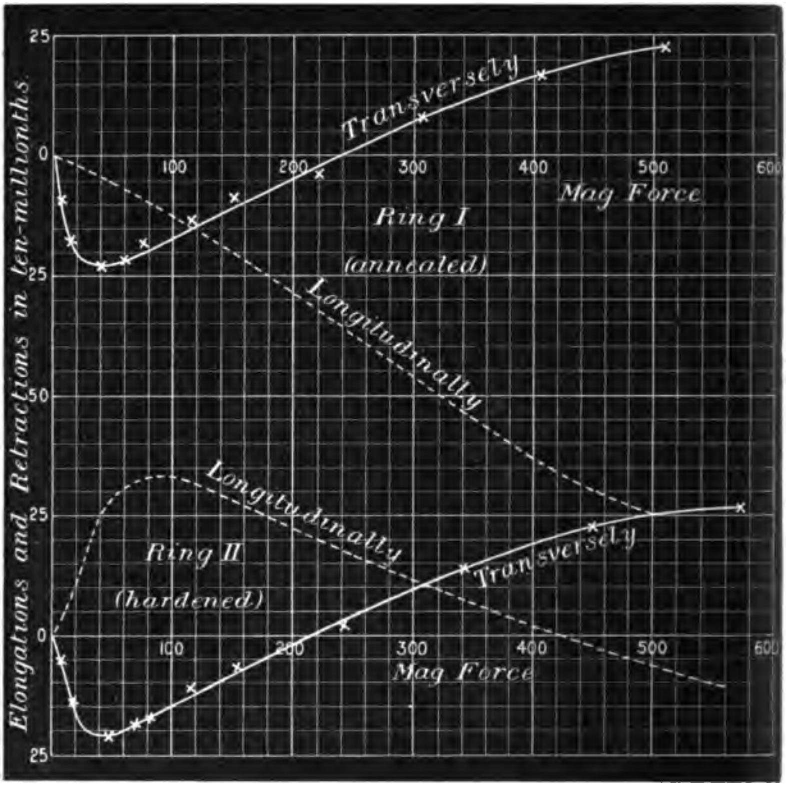
The annealed ring will, as before, be distinguished as Ring I, and the hardened one as Ring II.

Table I.

Ring I (annealed).		Ring II (hardened).	
Magnetising force, c.g.s. units.	Elongations in ten-millionths.	Magnetising force.	Elongations.
6	− 9	7	− 5
17	−18	18	−13·5
41	−23	47	−21
59	−21	69	−18
75	−17	82	−17
116	−13	117	−11
151	− 9	155	− 7
220	− 4	242	2·5
306	8	342	14
405	17	451	23
509	23	570	27

The changes observed in the widths of the two rings (transversely to the magnetisation) are indicated in Table I and in the curves of fig. 2. It will be seen that they are quite similar in the two cases,

FIG. 2.



The curves marked "longitudinally" relate to circumferential changes, *along* the lines of magnetisation.

Those marked "transversely" relate to changes in the width, *perpendicularly* to the magnetisation.

little or no effect being produced by annealing. Under gradually ascending forces both rings first become narrower, and then recover their original width, and ultimately become wider than when unmagnetised.

The only previous experiments that I know of relating to magnetic changes of dimensions in directions perpendicular to the magnetisation are those of Joule,* who used a piece of iron gas-piping 1 yd. long and ⅜ in. in mean diameter, having an insulated wire inserted into it, and bent over the sides, so as to form a magnetising coil of 1½ convolutions. The greatest current he used seems to have been about 12 ampères, and the magnetising force therefore about 8 c.g.s.

* Joule's 'Scientific Papers,' p. 263.

units. With this he found a contraction in the length of the pipe of 7 ten-millionths, a result which agrees very well with that obtained by myself for the same small magnetising force.

As was shown in my last paper, the effects along the lines of magnetisation are very different in the two rings. The annealed ring (Ring I) begins to contract circumferentially with the smallest forces, and continues to contract with the large ones; while the hardened ring expands with small forces and contracts with large ones. These effects are indicated in the figure by the dotted curves.

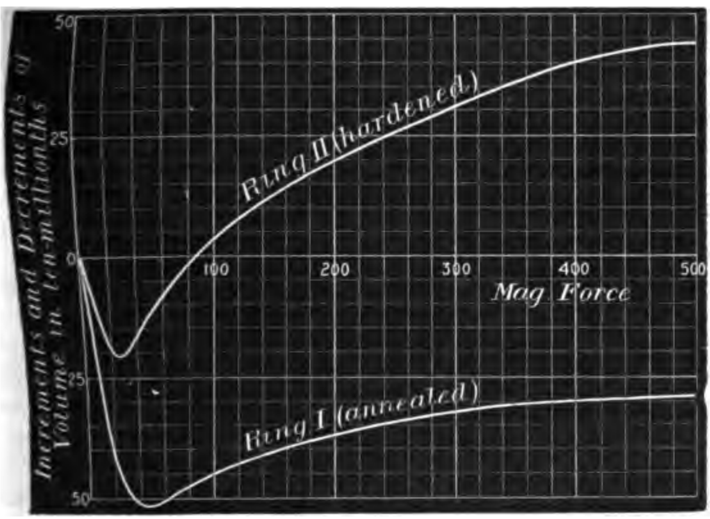
By combining the results of the old and of the new experiments we can ascertain the nature of the changes produced by magnetisation in the volumes of the rings.

If k = elongation (+ or −) along the lines of magnetisation,

l = elongation (+ or −) transversely to the lines of magnetisation,

then the increment or decrement of volume when the ring is magnetised is approximately* $k+2l$.

FIG. 3.



From the two sets of curves in fig. 1 corresponding values of k and l can be found, and thence the changes of volume may be deduced. These are given in Table II and fig. 3, which show that the volume of the annealed ring is rather suddenly diminished by a small magnetising force, passes a minimum under a force of about 50 units,

* Neglecting l^2 and products of k and l.

Table II.

Magnetising force, in c.g.s. units.	Increments and decrements of volume in ten-millionths.	
	Ring I (annealed).	Ring II (hardened).
10	−27	−11
20	−42	−20
30	−47	−20
40	−51	−17
60	−51	− 9
80	−48	− 2
100	−46	3
140	−42	11
180	−39	17
220	−37	22
260	−35	26
300	−32·5	30
400	−30	40
500	−29	44

and then slowly increases, until, with a force of 500 units, it is about 30 ten-millionths less than at starting. The unannealed ring also at first suffers diminution, but its original volume is recovered with a force of about 90 and with higher values is increased.

The behaviour of this latter ring may be regarded as probably similar to that of the great majority of rods and rings, the annealed ring used in these experiments being the only specimen of iron that has yet been found to contract along the lines of magnetisation with the smallest forces that produced any effect at all.

Experiments upon the volume changes produced by magnetisation have been previously made by Joule, Barrett, and Knott.

Joule* concluded that the volume of an iron bar was altogether unaffected by magnetisation, even though the magnetising current which he employed "was quite equal to saturate the iron." It was at that time believed that "saturation" was produced by a force of from 80 to 100 units, and, assuming that Joule's force was of about that value, an inspection of the curve for unannealed iron in fig. 3 will show the probable reason of his having failed to detect any change of volume. There is, in fact, none at all with a force of about 90 units.

Barrett,† experimenting in the same manner as Joule, "enclosing the bars in a vessel of water terminating in a capillary tube, and

* Joule's 'Scientific Papers,' p. 236.
† 'Nature,' vol. 26, p. 485.

surrounding the vessel by a powerful magnetising helix," also obtained a negative result, perhaps for the same reason.

Knott's* experiments were made with hollow iron tubes, 45·7 cm. in length, 3·84 cm. in external diameter, and of different bores, ranging from 0·7 cm. to 3·19 cm. "Each tube was closed below, and into the upper end a nut screwed tightly, through a perforation in which issued a fine capillary glass tube. The nut was adjusted under water, so that the whole of the interior space of the metal tube was filled with liquid, and also part of the glass tube. When the tube was set vertically in the heart of the magnetising coil, the changes of volume were measured by the motions of the liquid meniscus in the capillary tube." "A few experiments were made on the external change of volume of a few of the tubes, which were enclosed in a thin-walled brass tube. The brass tube yielded because of its thinness, so that the results were not certain. But there was no doubt that with the specimens of iron tried there were large changes of volume."

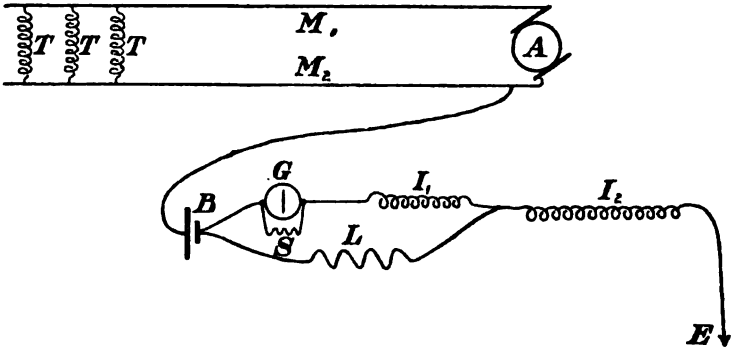
The changes observed by Knott in the interior volume appear in the case of a tube of large bore to have been of the same nature as those in my unannealed ring; while with a tube of smaller bore they rather resembled the changes exhibited by the annealed ring. His published investigations are, however, only of a preliminary character, and it is not at present possible to make a satisfactory comparison between his results and my own. But he was undoubtedly the first to show that magnetisation is generally attended by considerable changes of volume.

III. "Note on the Possibility of obtaining a Unidirectional Current to Earth from the Mains of an Alternating Current System." By Major P. CARDEW. Communicated by LORD KELVIN, P.R.S. Received May 10, 1894.

In carrying out some tests on the high-pressure alternating current system of the Metropolitan Electric Supply Company Ltd., of a combination intended to act as an indicator of leakage to earth, the existence under certain conditions of an excess of current in one direction to earth by leakage through the dielectric of the cables, or through small faults therein, has been demonstrated. The combinations and connexions used are shown in fig. 1, where A is the alternating current generator, M_1 and M_2 the distributing mains, TT the transformers, B a battery of a few Leclanché cells, G a

* 'Edin. Roy. Soc. Proc.,' 1891, p. 315; 1892, pp. 85, 249; 'Brit. Assoc. Rep.,' 1892, p. 659. The quotations are from the latter.

FIG. 1.



sensitive D'Arsonval reflecting galvanometer, S its $\frac{1}{9}$ shunt, I_1 and I_2 impedance coils, calculated to pass a current of less than 0·005 ampère with the whole alternating pressure in use on the system between the terminals, L a non-inductive resistance formed of four 50 C.P. 50-volt. incandescent lamps in parallel, E a connexion to the iron water-pipes supplying the station.

The object sought to be attained by the use of this arrangement was to obtain an indication of any leakage on the alternating system by a method which would be unaffected by the capacity effect of a large system.

It is intended to substitute for the D'Arsonval galvanometer used in these tests, a form of siphon recorder, so as to obtain a continuous record of leakage.

In the first tests, made on the 25th April, 1894, the mains in connexion consisted of eleven circuits all connected to one machine.

The pressure in the alternating circuit was rather greater than 1000 volts, and about half this pressure was indicated by an electrostatic voltmeter between M_2 and earth throughout the experiments.

The battery used was six cells, and the following deflections were obtained.

With +^{ve} pole of battery to the mains ..	20 to left.
With −^{ve} " " " ..	140 to right.
With battery out of circuit	48 "

Various modifications were tried, but in all cases the results showed an apparent electromotive force of from 5 to 6 volts, tending to cause a flow of positive electricity to the water-pipe earth.

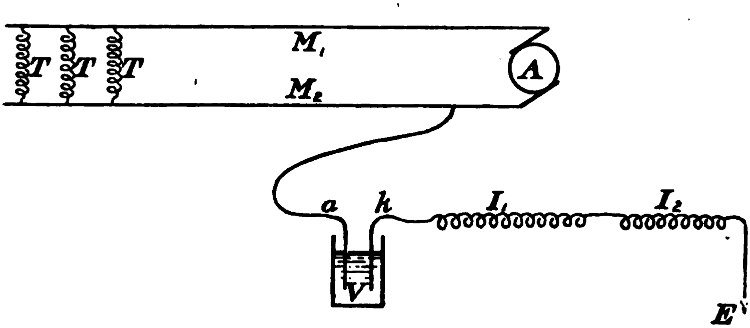
In order to settle this question, a small copper voltameter, consisting of two No. 40 S.W.G. copper wires in $CuSO_4$ solution, was inserted in place of the galvanometer and the shunts were removed.

In 2 hours and 10 minutes after connexion the wire connected to the mains was so far eaten through that it dropped off, while that connected to the water-pipe earth was visibly thickened by a deposit of copper. The gauge of the wires before and after this experiment was approximately as follows :—

Original gauge of each	0·005 inch.
Gauge of anode after experiment....	0·002 „
Ditto of kathode....................	0·0069 „

The connexions are shown in fig. 2, where V is the voltameter cell, *a* the wire which acted as anode, *k* that which acted as kathode.

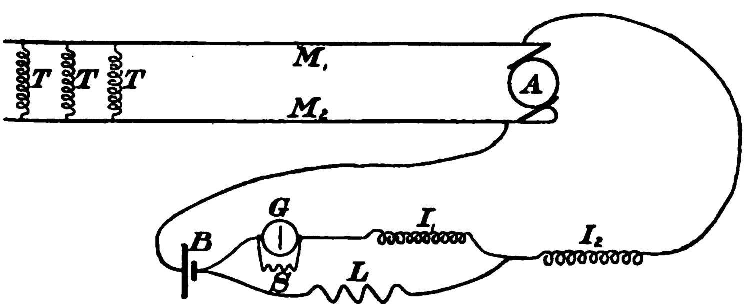
FIG. 2.



On the 2nd May, 1894, some further tests were taken.

In the first place a connexion to the opposite main was substituted for the connexion to the water-pipe, as shown in fig. 3.

FIG. 3.



This connection gave a deflection of about 235 to either side, according to the direction of the battery E.M.F., and absolutely no deflection without the battery.

On reverting to the connexions of fig. 1, a deflection of 130, gradually falling to 98 in 10 minutes, was obtained without any battery; and five Leclanché cells, connected $+^{ve}$ to mains, exactly reduced the deflection to zero.

The deflections obtainable with the connexions of fig. 1 without battery for varying lengths of street mains in connexion with the machine were then found to be as follows.

With the trunk main to Manchester Square Station alone, disconnected at the far end, no deflection was obtained:—

Adding	No. 10 circuit,	deflection of	15
„	No. 11 „	„	25

(with a sudden rise to 70 and then a fall to 20).

Adding No. 12, no increase of deflection

(this is a small circuit for station lighting only).

Adding	No. 7 circuit,	deflection of	35
„	No. 6 „	„	40
„	No. 5 „	„	48
„	No. 4 „	„	63
„	No. 3 „	„	70
„	No. 2 „	„	90
„	No. 1 „	„	115

In all cases the first deflection was a few scale divisions greater than that after a few minutes.

The effect of an artificial leak of about 103,360 ohms resistance, consisting of a pencil of graphite mixed with clay or cement and connected to the water-pipes and to the main M_1, was then tried.

This produced no effect on the deflections without a battery, but slightly increased the deflection with a battery and with all mains connected.

When tried on the trunk main alone with the lamps removed, galvanometer unshunted and six Leclanché cells, the deflection was increased from about 23 to 360.

With seven Leclanché cells (say 10 volts) and the impedance coils and shunts as shown in fig. 1, a deflection of four scale divisions was produced when the circuit was completed through the resistance of 103,360 ohms in place of the mains and earth; this gives an indication of the sensibility of the arrangement.

The explanation of the results obtained appears to be that when the cables are charged with positive electricity the polarisation produced is sufficient during the time of one alternation to considerably increase the resistance of the slight leakage to earth by the formation, probably, of a film of oxides; this obstruction is cleared

off by the succeeding wave of negative charge, which, as is well understood, opens the leak. The time of an alternation is, however, quite insufficient to produce any such effect on the water-pipe earth, and, in consequence, the net result is a passage of negative electricity to earth through the cables, and of the corresponding positive quantity to earth by the water-pipes.

The maximum effect that has been observed so far amounts to an apparent E.M.F. slightly exceeding 10 volts, with the eleven circuits connected to one machine, but it appears that a greater effect would be produced by still further increasing the length of mains in connection.

IV. "The Effect of Mechanical Stress and of Magnetisation on the Physical Properties of Alloys of Iron and Nickel and of Manganese Steel." By HERBERT TOMLINSON, B.A., F.R.S. Received May 7, 1894.

(Abstract.)

The author has examined the principal physical properties of three alloys of nickel with iron and of the non-magnetic manganese steel of Mr. Hadfield, together with the effects of mechanical stress and magnetisation on these properties. The three nickel-iron alloys contain 22, 25, and 30 per cent. of nickel, and are designated Specimens D, E, and F respectively; they are in the form of thin wires, and similar specimens have been previously tested by Dr. John Hopkinson for the effect of change of temperature on their magnetic properties.* Specimen F practically loses its magnetic susceptibility at a temperature below 100° C., but regains it again on cooling to the temperature of the room. Specimens D and E are magnetic in the hard-drawn condition, but become non-magnetic when heated above 600° C. They do not, however, like Specimen F, regain their magnetic susceptibility when cooled to the ordinary temperature of the room, but can be made to do so, either by the process of wire-drawing, or by cooling them a few degrees below 0° C. Tables I and II contain the values of the principal physical constants of the three nickel-iron alloys, of Hadfield's non-magnetic manganese steel, and of nickel and iron. The former of these two tables relates to the substances in the hard-drawn condition in which they were received by the author, and the latter to the same substances after annealing, so that by a comparison of the two tables, the effects of the permanent strain resulting from wire-drawing may be seen. These effects are in some instances of the same nature for the nickel-iron alloys as for nickel and iron. Thus the density of all the specimens is diminished by

* 'Roy. Soc. Proc.,' vol. 48, pp. 1—13.

wire-drawing, the simple rigidity is also diminished, and the internal molecular friction, as inferred from the logarithmic decrement of arc of torsionally oscillating wires, is considerably increased. On the contrary, whilst the specific resistance of iron, nickel, and manganese steel is increased by wire-drawing, that of the iron-nickel alloys is largely diminished. Again, whilst wire-drawing largely decreases the magnetic permeability of iron, nickel, and iron alloyed with 30 per cent. of nickel, it makes the alloys containing 22 and 25 per cent. of nickel to considerably appreciate in permeability, and, in fact, in a great measure transforms them from the almost non-magnetic to the magnetic condition. Dr. John Hopkinson has shown that a similar result can be obtained by cooling the alloys several degrees below 0° C. The magnetic properties of manganese steel cannot, however, be restored by the process of wire-drawing.

As regards the physical properties themselves, apart from the effects of stress or strain on these properties, all the nickel-iron alloys here examined have considerably less longitudinal and torsional elasticity than the pure metals nickel and iron; they also have considerably less internal friction. This last is very conspicuously the case with the alloys containing 25 per cent. nickel, which in the annealed non-magnetic condition has an internal molecular friction less than one-fourth of that of iron or nickel. For magnetising forces extending from 0·8 to 2 C.G.S. units, the alloy containing 30 per cent. of nickel is possessed of much greater magnetic permeability than iron, but for large forces the iron is superior. The other two nickel-iron alloys are, when in the annealed condition, almost as non-magnetic as Hadfield's manganese steel.

The temporary effects of longitudinal mechanical traction and magnetic stress on some of the physical properties are exhibited in Table III. It will be noticed that longitudinal traction produces on both the specific resistance and the thermo-electric height of the alloys of nickel with iron, an effect which is intermediate to that produced on the pure metals nickel and iron. The magnetising forces employed ranged for the most part between 40 and 80 C.G.S. units, and the results obtained refer only to the elastic effects of the force, which effects are approximately proportional to the force itself. These elastic effects are much greater in proportion to the change of magnetic induction than are the residual effects. The change of both specific resistance and thermo-electric height produced by a C.G.S. unit of magnetic stress, is in all cases enormously greater than that produced by a C.G.S. unit of mechanical stress.

The effects of mechanical stress on the magnetic permeability of all the different iron-nickel alloys are fully exhibited in this paper. Speaking roughly, they lie between the corresponding effects for the pure metals iron and nickel.

Table I.

The Physical Constants of the Specimens in the Hard-drawn Condition.

Specimen.	Density.	Young's modulus in grams weight per sq. cm.	Modulus of simple rigidity ditto.	Logarithmic* decrement of arc of a torsionally oscillating wire of the specimen.†	Specific resistance at 20° C. in C.G.S. units.	Thermo-electric height at 20° C. in C.G.S. units.‡	E.M.F. in C.G.S. units at 20° C. of a cell formed of the specimen, iron wire, and water.	Magnetic induction in C.G.S. units for a magnetising force of 54·7 C.G.S. units.
D	7·794	1590×10^6	529×10^6	0·000809	47980	−1931	-443×10^5	4540
E	7·930	1509	547	0·000674	60944	−1451	−363	6974
F.............	7·916	1428	527	0·000762	55080	− 434	−242	3661
Manganese steel	7·717	1449	675	0·000407	74840	− 718	− 5	197
Nickel	8·707	2271	723	0·002005	17570			
Iron	7·630	1862	704	0·001214	12090			

* Logarithms to base 10.

† A measure of the internal molecular friction.

‡ The convention as to sign is the same as that adopted by Professor Tait. According to this convention the current flows from the greater thermo-electric height to the less through the *cold* junction.

Table II.

The Physical Constants of the Specimens in the Annealed Condition.

Specimen.	Density.	Young's modulus in grams weight per sq. cm.	Modulus of simple rigidity ditto.	Logarithmic decrement of arc of a torsionally oscillating wire of the specimen.	Specific resistance at 20° C. in C.G.S. units.	Thermo-electric height at 20° C. in C.G.S. units.	E.M.F. in C.G.S. units at 20° C. of a cell formed of the specimen, iron wire, and water.	Magnetic induction in C.G.S. units for a magnetising force of 52·3 C.G.S. units.
D	7·917	1626×10^6	596×10^6	0·000411	75080	−1011	-98×10^5	307
E	8·045	1555	547	0·000212	78723	− 731	−149	189
F............	8·000	1328	527	0·000595	87930	− 380	−155	5857
Manganese steel	7·740	1516	675	0·000254	72913	− 731	+185	84
Nickel	8·739	2175	723	0·000852	17360	−2200		
Iron	7·759	1981	704	0·000914	10740	+1862	....	16160

Table III.

Specimen.	Alteration of specific resistance per unit produced by a longitudinal traction of 1 gram weight per sq. cm.	Alteration of specific resistance per unit produced by a longitudinally magnetising force of 1 C.G.S. unit.	Alteration of thermo-electric height per unit produced by a longitudinal traction of 1 gram weight per sq. cm.‡	Alteration of thermo-electric height per unit produced by a longitudinally magnetising force of 1 C.G.S. unit.‡
D, 22 per cent. nickel	$+ 7 \cdot 53 \times 10^{-10}$	$+ 3 \cdot 06 \times 10^{-6}$*	-27×10^{-10}*	$- 38 \cdot 9 \times {}^{-6}$*
E, 25 per cent. nickel	+ 6·64	+ 1·48*	−21*	− 38·3*
F, 30 per cent. nickel	− 1·34	+ 4·56*	−30*	−155·0*
Manganese steel..........	+ 11·30	+ 0·03*	+ 5*	
Nickel	−40·80	+80·70	− only qualitative result	− only qualitative result
Iron....................	+13·23	+23·35	+22*†	+ 68·6*†

* Specimens thus marked were in the hard-drawn condition; the others in the annealed state.

*† Piano-steel.

‡ A minus sign in these columns means that the thermo-electric height, whether originally positive or negative, is *numerically* decreased.

V. "Propagation of Magnetisation of Iron as affected by the Electric Currents in the Iron." By J. HOPKINSON, F.R.S., and E. WILSON. Received May 17, 1894.

(Abstract.)

Consider a solid, cylindrical electromagnet, it is well known that, in reversing the magnetising current, the induction does not instantly reverse, but a certain time elapses before it again attains its full value, that it reverses at a later time at the centre of the core than near its surface, and that the delay in reversal near the centre is due to the electric currents induced in the iron. The object of the present paper is to investigate these effects.

The magnet experimented upon had a diameter of 4 inches, and formed a closed magnetic circuit. Through a part of its length the cylinder of 4 inches diameter was formed of an iron core surrounded by two concentric, closely fitting tubes. Exploring coils of fine copper wire were bedded in the iron between the surfaces of the tubes. The currents induced in these exploring coils were observed when the current in the main coil of the magnet was reversed. These currents in some cases last for over half a minute.

Inferences can be drawn from these results as to the behaviour of other diameters than 4 inches. Comparing two cylinders of different diameters, similar events occur, but at times proportional to the squares of the diameters of the cylinders. From this consideration and the experiments, a judgment is formed as to the effects of local currents in the cores of transformers and of the armatures of dynamo machines.

VI. "On Rapid Variations of Atmospheric Temperature, especially during *Föhn*, and the Methods of observing them." By J. Y. BUCHANAN, F.R.S. Received May 29, 1894.

The variation of the temperature of the air in the course of a day is a matter of familiar observation. It depends in the first instance on the relative positions of the locality and the sun. The temperature is generally highest a short time after the sun has attained its greatest altidude above the horizon, and it is lowest some time after it has attained its greatest depression below the horizon. Observations made at regular intervals over the twenty-four hours show a more or less regular rise of temperature during the early part of the day and a similar fall of temperature during the latter part of the day and the evening. When the interval between the observations is

diminished the regularity of the march of temperature is found to diminish also, but the great variability of the temperature of the air is best shown by the curve drawn by a recording thermometer of sufficient sensibility combined with a clock movement of suitable velocity. Such an instrument draws a sinuous line which is generally smooth during the night and serrated during the day. The shape and the crowdedness of the teeth on the serrated daylight portion of the line have a close connection with, and are to a certain extent an indication of, the character of the existing weather. In general the indented character of the daylight curve is an indication of the disturbing influence of the sun on the equilibrium of the atmosphere which continues just as long as he is above the horizon; after sunset the atmosphere quickly reverts to a state of greater stability. It is obvious, therefore, that the indented character of the daylight curve indicates not only changes of temperature in the air but also motions and changes of motion in it. These motions are generally vertical and too subtle and local to be observed with an anemometer. In the course of frequent observations in the open air and under varying circumstances, I have many times had occasion to remark these rapid oscillations of temperature and at the same time to deplore the difficulty of accurately measuring them. It is principally with the view of directing attention to this instrumental difficulty that the following observations are put together. At the same time, though few in number, they have to do with a very remarkable species of weather, known by its Alpine name of *Föhn*. It has been most observed in the valleys stretching in a northerly direction from the main summit line of the chain of the Alps and takes the form of an abnormally warm wind blowing from the mountains towards the plain. It has largely occupied the attention of continental meteorologists, and more particularly it has been the subject of exhaustive investigations by Hann, who has shown by very strong evidence that its high temperature must be due to its compression in descending from a great altitude. In the descriptions of the Föhn, attention is almost exclusively directed to the high average temperature of the air, and no mention is made of their extraordinary variations, although every observer must have noticed them. They are so great as to be recognised at once by the sensations and at the same time so rapid as to elude almost every other method of estimation or measurement. It has also, I believe, not been before remarked that the true Föhn occurs in our own country and with its characteristics quite as well marked as in Switzerland. It is sometimes supposed that a great absolute height of mountain chain is required for its production; but this is not so. A relative height of 1,000 to 1,200 metres is quite sufficient for its production; and this is equally available on the west coast of Scotland and on the northern slopes of the Alps.

The observations were made in the summer of 1893, which was abnormally warm all over the north of Europe. In the beginning of July I observed the Föhn at Fort William, and in the latter part of August in the upper Engadin, and more particularly in the valley occupied by the Morteratsch glacier. Besides the observation of the varying temperature of the air itself, the investigation of the temperature gradient set up between the melting ice surface of the glacier and the hot winds blowing over it presented considerable interest. The curious fact was observed that while the hot wind was blowing over the glacier and melting the surface in abundance, the temperature of the air, as close to the ice as a thermometer could be applied without touching the ice, was never lower than 5·5° C.

In the beginning of July at Fort William the weather was very warm, and in the midst of the very warm air still hotter blasts made themselves felt from time to time. The sensation was much the same as is produced when, on the deck of a steamer, the air passing the funnel strikes the face. These hot blasts lasted only for one or two seconds, and repeated themselves every minute or two. Their effect on a thermometer, freely exposed in the shade, was to keep the mercury in a constant state of motion, the temperature rising often more than 1° C. in a minute, and falling again as much. The thermometers in the screens were also a good deal affected, though not nearly to the same extent as the freely exposed ones. The recording instruments, the clock motion of which was not sufficiently quick to draw the record out into an indented line, showed a broad band which measured the amplitude of the excursions of the instrument, though by no means the amplitude of the oscillations of the temperature of the air. This phenomenon was particularly observed on the 8th July, 1893, when I was employed the greater part of the day in making evaporation experiments. It was very warm, as the following observations of the thermometers in the large observatory screens will show:—

Hour...............	9 A.M.	10 A.M.	Noon.	2 P.M.	4 P.M.
Dry bulb (C.°)	20·1	22·4	24·9	23·8	18·9
Wet bulb (C.°)	17·7	17·3	18·2	17·7	16·6
Vapour tension (mm.)	13·5	11·5	11·5	11·3	12·6
Relative humidity....	77	58	49	52	77

It was during the heat of the day, from 10 A.M. to 2 P.M., that the hot puffs made themselves most felt; but I found it impossible to measure their temperatures, owing to the thermal inertia of the thermometers. The puffs lasted not longer than one or two seconds,

and their temperature, to judge by the sensation, was rather higher than that of the body. The thermometers had only begun to rise when the heating ceased, and they fell back again. From the figures in the above table, it will be seen that the temperature of the air at noon reached 24·9° C., a very high figure for a station in nearly 57° north latitude. Along with the great rise of temperature there is a fall of absolute as well as of relative humidity, indicating that the air has come from a greater altitude. Attempts to measure the actual temperatures of the hot puffs gave no satisfactory result. I am much obliged to Mr. Omond and the staff of the Fort William Observatory for their courteous assistance while making these observations.

Later in the year, in the middle of August, I visited the upper Engadin, and stayed for some weeks at Pontresina. Here, as elsewhere the weather was very warm, and I was much struck by observing the same blasts of hot air as I had experienced in Scotland. The general characteristics of the weather were the same, and the temperature of the air in the valley rose nearly as high as it had done at Fort William.

On the 18th August I went for an excursion on the Morteratsch glacier with a guide. On my remarking the hot puffs of air, which were much more striking on the ice than on the land, he said it was the Föhn, of which he considered them a characteristic. The sun and the hot wind were causing an enormous amount of surface melting of the ice, and having a thermometer with me, I took the temperature of the air by whirling at a height of about 1 m. from the ice, and found it 12·0° C.; the wet bulb was 5·0°, so that the vapour tension was 2·3 mm., the relative humidity 22, and the dew point −8·6° C. The great dryness of the air will be remarked. I then swung the thermometer in a conical path as close to the ice as possible, and the temperature of the air was 10·0° C. Being astonished to find so high a temperature so near the ice, I put the bulb of the thermometer into a crack in the ice, so as to be below the level of the surface of the ice, and its temperature only went down to 7·5° C.

All the temperatures were taken with a mercurial thermometer, which was whirled at the end of a string so that its velocity was about 6 m. per second. It was not protected in any way, so that the temperatures observed with it are not free from a certain error due to radiation and reflection, although it was always shaded from the direct sun. These errors are not usually great with a whirled instrument, and most of my observations have to do with differences of temperatures observed with the same instrument and under similar circumstances. On the glacier the thermometer, when whirled, was not apparently affected by radiation or reflection from the ice, and only very slightly by that from the sun. On land I

remarked that the greatest disturbing effect is produced by sunlight reflected from grass. If the thermometer was whirled in the shade of a north wall with a grass field or hill-side close by, the thermometer would be immediately affected to the extent of one to two degrees, according as the sun shone on the grass or was obscured by a cloud. The effect was immediate the moment the sun came out; sunlight reflected from rocks and light-coloured surfaces did not produce the same effect.

On the 19th August I returned to the glacier. At 11 A.M. in the valley below the glacier I found the temperature of the air 22° C., and the wet bulb 12°·5, whence the vapour tension is 5·0 mm., and the relative humidity 26. In determining the temperature of the air by whirling the thermometer I found variations of as much as 2°. The hot puffs of air made themselves felt most markedly, and showed that the real variations of the temperature of the air were much greater than the thermometer showed. At 1 P.M., on the hill-side, to the west of the tongue of the glacier, and at a height of about 2,100 m. above the sea, four good observations of the temperature were made, giving 17°·5, 18°·0, 19°·5, and 19°·0; they are all equally trustworthy, and represent the average temperatures of the air during the minute, or minute and a half, that the thermometer was whirled. The mean of these values, 18°·5 is taken as the temperature of the air. For determining the temperature of the wet bulb the bulb of the thermometer was wrapped round with one thickness of Swedish filtering paper thoroughly moistened, and the thermometer was whirled as before and until the temperature ceased to fall, it then stood at 9°·5. Still higher up the hill at an altitude of 2,250 m., the temperature of the air at 2 P.M. was 18°·5 C. Having returned to the same spot where the observations were made at 1 P.M. the following air temperatures were observed:—between 2.40 and 2.46 P.M., 17°·5, 18°·0, 17°·5, 17°·0, 17°·3, 17°·1; mean, 17°·4; and between 2.50 and 2.54 P.M. 16°·5, 16°·5, 16°·7, and 16°·5; mean, 16°·55. The mean of the two sets is 17°·06. Again it must be repeated that each of these individual observations is a faithful indication of the average temperature of the air in which the thermometer was whirled, and in so far as its sensibility enabled it to assume the same temperature as the air. From this spot I descended to the glacier and went up it until I got to a position which, judging by the eye, was at the same height as the station just left on the mountain side, and about one kilometre distant from it in a straight line. The weather was rapidly getting colder, the sky being covered with the characteristic Föhn cloud. The wind was fresh down the glacier, which made the exposure of the thermometer easy and good. The hot Föhn puffs were also very striking. The thermometer was first swung exposed to sun and wind, showing

temperatures varying from 10°·5 to 11°·2, the mean being 10°·8 C. Swung in my own shadow, but exposed to the wind, the temperature was 9°·8. The wet bulb was 4°·7, showing a relative humidity of 37. The thermometer was now exposed, both wet and dry, in a horizontal position with the bulb at a distance of about 2 cm. from the ice, on the top of one of the superficial ridges of the glacier, and fully exposed to the wind, though shaded from the sun. The observed temperatures were: dry, 6°·6 C.; wet, 3°·7; relative humidity, 58°·5. The exposure of the thermometer was as good as could be desired, and, with the fresh breeze blowing, it was thoroughly ventilated. I was again much struck with the highness of the temperature of the air almost in actual contact with the ice. The observations at 1 m. and 2 cm. from the ice were repeated, giving substantially the same results—at 1 m., dry bulb 10°·2, wet 5°·1; at 2 cm., dry bulb 6°·8, and wet 3°·2. The hot Föhn puffs were more striking on the ice than on the land, owing to the greater difference between their temperature and that of the surrounding air. At 4 P.M. I left the ice and returned to the station of 1 o'clock on the hill-side, and took the temperature at 4.35 P.M.—dry bulb 16°·0, wet 8°·0, relative humidity 24°·5. At the station in the valley below the glacier the temperature was at 5.45 P.M., dry bulb 16°·4, wet 11°·8, and relative humidity 56. These observations, besides showing the remarkable conditions of the air over the glacier, indicate the fineness and warmth of the weather which prevailed.

On the 21st August another series of observations was made at the stations on the land and on the ice. The breeze on the ice was not so steady or so strong as on the 19th, and about 5 o'clock in the afternoon there was a heavy squall of rain and thunder. The same hot Föhn puffs made themselves felt as before, without there being any means of measuring their temperature. Their duration at their maximum temperature was never more than a few seconds, during which but little effect was produced on the thermometer. It occurred to me that the only way of gaining a knowledge of the temperature of these puffs of air would be by comparing the rapidity with which the thermometer moved when exposed to a known difference of temperature, with that observed in the puffs. A number of observations was made with this view, by warming the thermometer and noting its rate of cooling in air of known temperature. The reverse procedure was also followed on the ice. The thermometer was cooled by being laid close to, but not touching, the ice, it was then quickly raised to a height of 1 metre, and its rate of change of temperature observed. In this way it was found that for an initial difference of 4° the thermometer required 10 seconds to rise 1°, for a difference of 3° 12 seconds, and for a difference of 2°·5 16 seconds. These ratios were observed in the open air, and under the circum-

stances where the hot puffs are observed. Unfortunately, owing to an accident to the thermometer, very little use could be made of them. Where the rate of change of temperature of the thermometer is used to determine the temperature of the air, the movement of the air must be measured or estimated. The observations made on the 19th and 21st August are given in Table I.

Table I.—Temperature Observations at Equal Altitudes on the Morteratsch Glacier, and on the Mountain west of it.

	Thermometer.		Diff.	Vapour tension.	Rel. hum.	Dew point.
	Dry.	Wet.				
19th August, 1893.	C°.	C°.	C°.	mm.	p. c.	C°.
Land station, 2.45 P.M.	17·1	8·6	8·5	3·2	22	−5·0
„ „ 4.35 „	16·0	8·1	7·9	3·2	24	−4·7
Mean....	16·55	8·35	8·2	3·2	23	−4·85
Ice station, 3.20 P.M.	9·8	4·7	5·2	3·26	36	−4·4
Height 1 metre, 3.55 „	10·2	5·1	5·1	3·55	39	−3·5
Mean....	10·0	4·9	5·1	3·40	37·5	−3·95
Ice station, 3.20 P.M.	6·7	3·7	3·0	4·2	57	−1·4
Height 0·02 m. 3.55 „	6·6	3·2	4·4	4·0	56	−3·0
Mean....	6·65	3·45	3·2	4·1	56·5	−2·2
21st August, 1893.						
Land station, 1 P.M.	14·5	7·5	7·0	3·5	29	−3·5
„ „ 3.45 „	14·3	8·0	6·3	4·2	35·0	−1·3
Mean....	14·4	7·75	6·65	3·8	32	−2·4
Ice station, 2.22 P.M.	9·85	5·6	4·25	4·2	47	−1·3
Height 1 metre, 2.54 „	11·0	7·0	4·0	5·1	52	+1·5
Mean....	10·43	6·3	4·13	4·6	50	+0·1
Ice station, 2.15 P.M.	7·3	4·0	3·3	4·1	54	−1·5
Close to ice, 2.40 „	5·5	3·2	2·3	4·2	65	−0·7
Mean....	6·4	3·6	2·8	4·2	59	−1·1

For comparison with the temperatures on the ice on the 19th, the mean of the observations on the land station at 2.45 and 4.35 P.M. is taken, and on the ice the mean of the observations at 3.20 and 3.55 P.M. The altitudes of the two stations were as nearly as possible identical, and they were not more than 1 kilometre distant from each

other. Considering the temperatures at a height of 1 m. there is a difference of 6°·5 between the land and the ice. The difference of vapour tension, 0·2 mm., is insignificant, and shows that substantially the air is the same. The dew point in both cases is several degrees below 0°, so that, on coming in contact with the ice, there would be evaporation from it. The evaporating power of the air may be represented by the difference between the tension of saturation and the actual vapour tension. It is very great on land, being 10·75 mm. at 16°·53 C., and it would rapidly evaporate water having that temperature. On coming in contact, however, with ice the air actually in contact, which alone comes under consideration, is first cooled to 0° C., which reduces its saturation tension to 4·6 mm., and the difference is only 1·4 mm. We see, however, that this has been sufficient to increase the absolute humidity of the air in close proximity to the ice. At 1 m. above the ice the air had an average temperature of 10° C.; at 2 cm. from the ice its temperature was as high as 6°·65 C., and the air in actual contact with the ice must have been at 0° C. Many observations have been made of the temperature of the air at different heights above glaciers, and, as might be expected, considerable differences have been observed; but I am not aware that any observations have been made on the air almost but not quite in contact with the ice, as are those which have been made at 2 cm. from the ice. The bulb was perfectly shaded from the sun but freely exposed to the wind, it was also fully exposed to any cold radiations from the ice There is, therefore, no doubt that 6°·65 was the temperature of the air passing the bulb of the thermometer. The vertical distribution of temperature shown by these figures is remarkable. From a height of 1 m. to within 2 cm. of the ice there is a gradient of 3°·4 per metre, in the remaining 2 cm. there is a gradient at the rate of 33° per metre; and, from various observations and considerations, it is probable that the moderate gradient is continued to within a millimetre of the ice, when it becomes precipitous. It is to be noted that the absolute humidity, as shown by the vapour tension of the air, has increased from 3·4 mm., at 1 m., to 4·1 mm., at 2 cm.; showing that ice is being evaporated and transferred from the glacier to the atmosphere. The wind was blowing freshly down the glacier, and its velocity was measured by noting the time which pieces of paper allowed to drift took to reach the ice, and then pacing the distance. The mean velocity was found to be from 8 to 10 kiloms. per hour.

The observations made on the 21st and on the 22nd confirmed those of the 19th. The same variability of the air temperature at the land stations was noticed. Between 12.55 and 1.6 P.M. the following temperatures were observed by whirling:—16°·2, 16°·2, 16°·0, 15°·5, 16°·0, 15°·5, 15°·0, 14°·2, 13°·8, 14°·0, 13°·5, 13°·5. These are all good observations, and represent real variations of the temperature, or rather

they indicate real variations of greater amount. Taking the mean of the last five observations, we have the temperature of the air 14°·0. The wet bulb was found at 1.15 P.M. to be 7°·5, giving a difference of 6°·5. On the glacier the air felt closer than on the previous occasion. The temperature at 1 m. was 11°·5, and at 2 cm. from the ice 7°·3. The difference 4°·2 is less than on the previous occasion. The wind was much less strong, and yet the temperature close to the ice is higher. The wet bulb, under the same circumstances, showed 4°·0. Five minutes later the dry bulb was observed at 1 m. 10°·2 and 9°·4, mean 9°·85. Another observation of the dry bulb at 2 cm. from the ice gave 6°·6. The interval between the bulb and the ice was now reduced to the smallest possible distance, about 2 mm. The wind fell very light, and the thermometer remained at 8°·0, when the wind returned it fell to 5°·8. The axis of the thermometer bulb would be about 5 mm. from the ice, and still the air is nearly 6° warmer than the ice. Another observation on the same conditions gave 5°·5. The wet bulb was now exposed, but it had to be kept about 5 mm. off the ice; it showed 3°·2. At 2.43 P.M. a great volume of warm air came down, and the wet bulb ran up to 4°·5 in three or four seconds. With the return of the breeze the wet bulb went back to 3°·0. The Föhn puffs were now very troublesome. At 2.52 P.M. the wet bulb at 1 m. was 7°·0; the dry bulb showed—at 2.54 P.M., 11°·0; at 2.55 P.M., 13°·5; and at 2.57 P.M., 14°·5. In one puff the thermometer was observed to rise one degree in eight seconds, which would make the true temperature of the air at the moment about 6°·0 higher, or 19°·5.

At 3.30 P.M. I returned to the land stations, and again found the same variable temperatures. Between 3.35 and 3.45 P.M. the temperature varied between 16°·0 and 13°·5. The following averages were taken :—

3.45 P.M., dry, 14°·3; wet, 8°·0; relative humidity, 35°.
4.0 „ „ 14°·0; „ 8°·5; „ „ 42°·5.

Taking the first of these and the observations at 1 o'clock, we have for the mean temperature of the air 14°·15, and the wet bulb 7°·75. On the ice we have—

At 1 m., dry bulb, 9°·85; wet, 5°·6, and
At 2 cm., „ 7°·3; „ 4°·0.

The difference in the temperature of the air at 1 m. is only 4°·3, and that between 1 m. and 2 cm. above the ice is only 2°·55, while the air at 2 cm. is 7°·3 warmer than the ice.

On the 22nd August, the observations on the ice were repeated with very much the same results. The temperature of the air ranged from 9°·0 to 9°·5 at 1 m., and was 5°·5 at 1 cm. from the ice.

The result of the few observations here quoted is to show that the air, which over land has a temperature of 15° to 20° or higher, in passing over a glacier is cooled to a comparatively slight degree. Although the air appears to be thoroughly mixed by its own motion, very sharp gradients of temperature are produced and maintained. The great and abnormal temperature of the air of the valley is kept up by the heat liberated by the compression accompanying the descent of local streams or striæ of air from high levels. These keep up an extra supply of heat over and above what is supplied by the direct radiation of the sun. The result is that the melting of the glacier in Föhn weather greatly exceeds that of even the hottest day of ordinary weather.

In order to convey a general idea of the climate in the neighbourhood during the period when my observations were made, I subjoin a table of the air temperatures observed at the Pfarrhaus in Pontresina three times daily, and obligingly supplied to me by Herrn Pfarrer Falliopi.

Table II.—Temperature of the Air at Pontresina.

Date.	Temperature of the air observed at					
	7 A.M.		1 P.M.		9 P.M.	
	Temp.	Diff. from mean.	Temp.	Diff. from mean.	Temp.	Diff. from mean.
1893	C°.		C°.		C°.	
August 15.....	4·7	−2·92	19·2	−1·26	10·0	−1·36
„ 16.....	5·9	−1·72	20·0	−0·46	10·8	−0·56
„ 17.....	7·2	−0·42	20·8	+0·34	11·8	+0·44
„ 18.....	8·2	+0·58	21·8	+1·34	12·8	+1·44
„ 19.....	8·6	+0·98	21·2	+0·74	12·8	+1·44
„ 20.....	10·0	+2·38	19·8	−0·66	12·6	+1·24
„ 21.....	7·6	−0·02	22·2	+1·74	10·2	−1·16
„ 22.....	8·2	+0·58	20·2	−0·26	10·2	−1·16
„ 23.....	6·9	−0·72	19·2	−1·26	12·8	+1·44
„ 24.....	8·9	+1·28	20·2	−0·26	9·6	−1·76
Mean......	7·62	..	20·46	..	11·36	..

In this table the very high temperature on the 18th, 19th, 20th, and 21st is very apparent. The Föhn prevailed during all these days.

On the 23rd August, which was a very warm day, I made a series of observations between Pontresina and the top of the Piz Languard, which is the highest peak on the ridge immediately behind Pontre-

sina, and is very easily accessible. It had been raining heavily in the night, so that in the early morning the air was rather cool; but the following observations made before starting up the mountain will show how rapidly the temperature was beginning to rise.

8.0 A.M.	Dry bulb, 10°·4;	wet, 9°·2.
9.10 „	„ 14°·8;	„ 11°·4.
10.0 „	„ 17°·0.	

At 10 A.M. I started up the mountain, following the excellent path which leads to the summit.

In the following table the temperatures observed at various stations are entered along with corresponding ones observed in the porch of the Hotel Reseg at Pontresina.

	Height above sea.	Time.	Temperature.		Difference.
			On mountain.	At hotel.	
	m.		°	°	
Pontresina ...	1800	10. 0	17·0		
	2100	10. 50	16·5	19·5	3·0
	2250	11. 5	16·5	20·0	3·5
	2370	11. 35	16·5	20·5	4·0
	2670	12. 0	14·5	20·75	6·25
	2790	12. 30	13·3	21·0	7·7
	2970	1. 0	14·0	21·5	7·5
	3180	1. 30	13·1	22·0	8·9
Summit......	3266	2. 10	11·0		
	—	2. 40	10·5	22·0	11·25

Excepting in the first interval the rate of fall of temperature between Pontresina and the station on the mountain is less than 1° per hundred metres. At the summit the mean temperature of the dry bulb was 10°·75, and of the wet bulb 6°·45, whence we have the vapour tension 4·5 mm. and the relative humidity 47. The weather was of the same kind as in the valley, abnormally warm, and the air very dry.

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The thermometer which was used in these observations was not very sensitive, and when it was broken I could only replace it by one which was considerably less so. They were, therefore, of no use for determining rapidly varying temperatures. The method indicated

above, whereby the temperature of the air is inferred from the velocity with which the thermometer rises or falls when immersed in it, either at rest or moving with a known speed, is in itself quite satisfactory. The difficulty in applying it is to ascertain the rate of motion of the air, because, other conditions being the same, the thermometer changes its temperature in proportion to the velocity of the air passing it. When the air has a horizontal motion it is called wind, and there are many instruments for its measurement; but there is probably nearly as much vertical as horizontal motion in the atmosphere, but it is seldom observed and not easily measured. In fact, a very good way of detecting these movements in air which, to the senses, appears to be motionless, is to observe the rate of cooling or heating of a thermometer in it. A thermometer similar to the one used in these investigations was carefully tested as to its rate of cooling, in connexion with a series of observations made in the winter.

Its rate of cooling was repeatedly determined in a room of constant temperature, and in the open air, when it was, to all appearance, motionless. Sometimes the rate of cooling in the open air was very nearly the same as in the room, but at other times it was much greater. It was never less. In four experiments, taking the same excess of temperature above the air, namely, 5°·5 C., the temperature of the thermometer fell by half that amount, 2°·75 C., in the room in 125 secs., and in the open air, which was apparently still, in 100, 70, and in 55 secs. The volume of the bulb of this thermometer, which was cylindrical, was 0·92 c.c.; it was rather sluggish.

Applying Leslie's rule for finding the "range" of the thermometer from the time it takes to cool to half the extent of the difference between its initial temperature and that of the air, we multiply it by 101/70. Leslie* defines the "range" of a thermometer or other body cooling to be the reciprocal of the fraction of the whole initial difference of temperature between the thermometer and the air, by which it cools in the first interval of time; or it is the time in which the thermometer would fall to the temperature of the medium, if, in each successive interval of time, its temperature had fallen by the same amount as in the first interval of time. The "ranges" of our thermometer cooling in the above conditions are found to be 180, 143, 100, and 80 secs. respectively.

Having recognised that, in the conditions under which he experimented, the refrigerant power of a stream of air is exactly proportional to its velocity, he gives† a formula for finding the velocity of the wind from the rate of cooling of a thermometer, or other similar

* 'An Experimental Inquiry into the Nature and Propagation of Heat,' by John Leslie, Edinburgh, 1804, p. 264.

† Page 283.

vessel, in it. If T be the "range" in still air, and t the observed "range," then the velocity of the wind is

$$v = \frac{20}{3} \cdot \frac{T-t}{t} \text{ in feet per second, or}$$

$$v = \frac{T-t}{t} \times 4\tfrac{1}{2} \text{ in miles per hour.}$$

Converting into metrical units we have

$$v = 2{\cdot}032 \frac{T-t}{t} \text{ in metres per second.}$$

If in our experiments we ascribe the whole difference in the rate of cooling in the room and in the open air to motion of the air, and apply Leslie's formula, we find that the air must have been passing the thermometer at the rate of 0·5, 1·6, and 2·5 m. per second respectively. On each occasion there was no perceptible horizontal motion of the air, and the differences in the rates of cooling observed may, in the absence of a better explanation, be held to indicate the presence of ascending or descending currents of probably very local character.

In the winter of this year I revisited the Engadin, and stayed for a fortnight at St. Moritz. As the room which I occupied faced due north the window of it was convenient for making observations of the temperature of the air. From the 24th February to the 3rd March I made every morning a series of observations of the temperature of the air, beginning when there was just light enough to read the thermometer, and continuing till between 8 and 9 o'clock in the morning. At first I took the temperature every minute, but finding the oscillations of temperature very great, I reduced the intervals to twenty seconds, and sometimes to fifteen seconds. The thermometer used was the one whose "ranges" in the still air of a room and outside have been given above. As before remarked, it is a sluggish instrument, yet the variations which it indicated in these short intervals of time were much greater than I could have anticipated. To print the observations *in extenso* would occupy too much space, but the striking features can be easily summarised. They are given in Table III. Excepting on the 26th February, when it was snowing all the morning, the observations embrace the interval of an hour or an hour and a half after sunrise. The time was devoted entirely to this object, and observations were made at as close dates as possible. Working alone, an interval of twenty seconds is quite convenient; shorter intervals cause hurry. The time immediately following sunrise is when one would expect the temperature of the air to rise continuously, if not regularly; but we see that so far from rising continuously and regularly the thermometer

Table III, giving Results of Observations of the Temperature of the Air and its Variations at St. Moritz.

Date, 1894.	Time of observation.	Limits of temperature.	Interval between observations.	Number of intervals in which the temperature was observed to			Total number of intervals.	Maximum rise or fall in any one interval.	
				Rise.	Fall.	Remain constant.		Rise.	Fall.
25 February	6.24 A.M. to 7.32 „	− 8°·0 − 5·0	60″	32	22	13	67	0°·51	0°·50
26 „	11.10 „ to 1.19 P.M.	+ 8·25 + 3·5	20	32	33	61	126	0·13	0·50
27 „	6.55 A.M. to 8.40 „	+ 1·75 + 5·35	20	80	43	45	168	0·37	0·47
28 „	7.0 „ to 8.2 „	− 6·48 − 1·0	20	103	37	45	185	0·25	0·20
1 March	6.30 „ to 7.30 „	− 4·25 − 2·1	15	93	68	80	241	0·25	0·20
2 „	6.38 „ to 8.6 „	− 6·23 − 2·03	13	158	118	131	407	0·23	0·18
3 „	6.30 „ to 8.6 „	− 6·55 − 1·68	20	120	89	77	285	0·28	0·20

rises, falls, and remains stationary quite irregularly. On some days, as on the 28th February, these irregularities are comparatively few; on others, as on the 1st and 2nd of March, they are numerous. The largest rise or fall in twenty seconds is 0°·5 C. From experiments in calm air outside and in still air in a room we find that for this thermometer to rise or fall 0°·5 C. in twenty seconds the temperature of the air around it must be from 2°·25 C. to 4°·65 C. hotter or colder than the thermometer. Taking even the lowest of these values, we see how great the possible error is in measuring the actual temperature of the air at any moment with a thermometer, and the error is the greater the more sluggish the instrument is. In Table IV the detailed observations are given for a few minutes on the 26th February, when the temperature was changing very rapidly. In the third and fourth columns the rise or fall of the

Table IV.—Temperature of the air at St. Moritz, observed at intervals of twenty seconds.

Date, 26 February, 1894.	Observed temperature.	Difference.		Corresponding difference of temperature of air.		Amended temperature of air.	Differences of amended temperatures.	
		Fall.	Rise.					
	T. C°.	−	+	−t.	+t.	T′ = T + t.	Fall.	Rise.
A.M. h. m. s.								
11 18 45	5·88	..	..	..	..	6·48		
19 5	6·00	..	0·12	..	0·60	6·60	..	0·12
25	6·12	..	0·12	..	0·60	6·72	..	0·12
45	6·25	..	0·13	..	0·60	6·25	0·47	
20 5	6·25	..	..	..	..	6·25		
25	6·25	..	..	..	..	5·25	1·00	
45	6·00	0·25	..	1·00	..	4·30	0·95	
21 5	5·62	0·38	..	1·70	..	3·37	0·93	
25	5·12	0·50	..	2·25	..	4·12	..	0·75
45	4·88	0·24	..	1·00	..	2·63	1·49	
22 5	4·38	0·50	..	2·25	..	2·13	0·50	
25	3·88	0·50	..	2·25	..	3·88	..	0·75
45	3·88	..	..	..	..	3·28	0·50	
23 5	3·75	0·13	..	0·60	..	3·75	..	0·47
25	3·75	..	..	..	..	4·37	..	0·62
45	3·88	..	0·13	..	0·60	3·88	0·49	
24 5	3·88	..	..	..	..	3·28	0·50	
25	3·75	0·13	..	0·60	..	3·15	0·13	
45	3·62	0·13	..	0·60	..	3·12	0·03	
25 5	3·50	0·12	..	0·50	..	3·00	0·12	
25	3·62	..	0·12	..	0·50	4·00	..	1·00
45	3·50	0·12	..	0·50	..	3·12	0·88	
26 5	3·50	..	..	..	..	3·50	..	0·38
25	3·50							

observed temperature is given. In the fifth and sixth columns the corresponding differences between the temperature of the air and that of the thermometer which would cause the observed rate of change of temperature are given; with these and the observed temperatures we obtain the amended temperatures of the seventh column. Although it was snowing on the 26th the air was perfectly still, and the rate of cooling corresponding to the "range" 80 secs. has been applied. Had the rate of cooling of the thermometer in the still air of a room been taken the difference between amended and observed temperatures would have been nearly twice as great.

It was interesting to know what could be obtained with a recording thermometer of ordinary type, and in Table V the results of some observations made in Cambridge with a Richard's recorder are given.

Table V, giving the Time in Seconds required by a Richard's Recording Thermometer to change its Temperature by 1° C. for a given Difference of Temperature between it and the Air.

Difference of temperature between thermometer and air at beginning of exposure.		12°.	11°.	10°.	9°.	8°.	7°.	6°.	5°.	4°.	3°.	2°.
Time in seconds required by thermometer to fall or rise 1° C. for above differences.	In the open air and fresh breezes.	20″	20″	25″	25″	30″	30″	30″	65″	90″	90″	240″
		..	..	..	..	..	35	45	120	130	150	300
		..	..	..	..	..	20	35	40	45	80	240
	Mean from curve.	20	22	24	26	28	30	35	52	84	140	250
	In still air in a room.	..	..	..	..	..	..	60	70	110	130	210
		..	..	..	..	..	..	..	90	100	300	450
		..	..	..	..	..	..	..	..	120	160	300
	Mean	..	..	..	..	..	..	60	80	110	180	320

The figures in this table are taken from the curves drawn by the instrument on a drum revolving once in forty-eight minutes. The instrument was allowed to take the temperature of the room, then exposed in the shade in the open air when a fresh breeze was blowing and allowed to remain there until it had taken the temperature of the air. It was then transferred to the room, and allowed to rise until it attained its temperature.

In this way two sets of curves were obtained, consisting of three curves in still air and three in a fresh breeze. The results are not very concordant, for, although the scale of time is very open—1 min. occupying 5 mm.—the temperature scale was very close, 1° occupying only 1 mm. The object, however, of the table is to show what can be expected from an instrument of the kind in the measurement of changes of temperature. The results obtained in the open air would necessarily vary somewhat, because, although a fresh breeze was blowing all the time, a fresh breeze varies in velocity.

In order to obtain the best results from a thermometer it should be exposed to uniform ventilation. This can only be effected by artificial means, and they necessarily tend to efface sharp variations of temperature. The arrangement adopted by Professor Assmann in his psychrometer for ventilating and exposing his thermometers ought to be suitable for this purpose. The current of air produced must be uniform, and the behaviour of the thermometer as regards rate of change of temperatures in the current produced must be accurately determined.

In Assmann's arrangement the thermometer is enclosed in a metal tube, consequently the diameter of the bulb, on which the sensitiveness depends, can be made smaller and its length greater than would be safe with an unprotected instrument. A mercurial thermometer, therefore, ventilated on Assmann's system, ought to be efficient for the measurement of temperatures changing with considerable rapidity.

Departing from the mercurial thermometer I have found the simple air thermometer very good for indicating and measuring quick variations of temperature. It has the advantage of lightness and cheapness. The form which I use is a glass bulb, of about 3 cm. diameter on a straight stem of about 10 cm. length. This can be attached to a U-tube of greater or less diameter, according as the differences of temperature to be observed are great or small. The U-tube has some coloured water as indicator, and the indications of the instrument are compared with those of a thermometer. As the instrument is only put together when it is wanted, the variations of barometric pressure do not affect it. It has the great advantage that it can be connected with a *tambour*, and thus be made to record. The sensitiveness of the glass air thermometer is about the same as that of a very fine mercurial thermometer made for me by Messrs. Hicks. The air thermometer, however, would be very much more sensitive if the ball were made of thin metal instead of glass.

There is a limit to the sensitiveness of all thermometers depending on the dilatation of a fluid, and I do not think that any such thermometer can be constructed which would give directly the true temperature of the air in the puffs of Föhn wind which we have been

discussing; by taking account of the rapidity of their movement they can be constructed to give the temperature inferentially. The only probable method of observing directly such rapid changes of temperature is by electric or thermoelectric methods. A thermoelectric junction is made of metals which conduct the heat rapidly, and as their mass can be made very small and their specific heat is low they can be made to follow the temperature of the medium in which they are immersed more closely than any other form of thermometric apparatus. The galvanometer necessary for measuring the currents produced is the inconvenient part of the apparatus, but I am informed by those familiar with such apparatus that a suitable instrument for use in the field could be constructed without difficulty.

Thermometers as Calorimeters.—If we know not only the rate of cooling of a thermometer, if we have the figure which, in Leslie's language, is called the "range," and if in addition we know the thermal mass of the bulb which is generally expressed by its "water value," the thermometer becomes an efficient calorimeter. It is a familiar observation that the thermometer and the senses frequently disagree about the warmth or coldness of the weather. This is because they measure different things. The thermometer measures the temperature of the air, the senses measure the heating or cooling power of the atmosphere, or the rate at which the body is called upon to receive or supply heat. The body is a calorimeter and not a mere thermometer. But with a knowledge of the constants above mentioned, the thermometer becomes also a calorimeter.

In connection with the melting of ice by the hot wind in the Engadin, and the corresponding abstraction of heat from the air, I made a number of experiments by whirling thermometers at various speeds in air of definite temperature, having previously warmed the thermometer to a higher temperature.

In order to give calorimetric expression to the result, and to express the heat exchange which had taken place, it was necessary to know the water value or thermal mass of the thermometer bulb. In similar experiments made by Leslie, he used a tin sphere 4 in. in diameter filled with water, of which it contained more than half a litre, and there was no difficulty in finding the thermal mass, as that of the thermometer was an insignificant fraction of it. With a mercurial thermometer, however, of ordinary type the glass envelope of the bulb is as important from a calorimetric point of view as the mercury contained in it; and it is impossible to know the proportions in which the two substances are present, except by weighing them in process either of construction or of destruction. The former of these processes was excluded, and I hesitated to adopt the latter before some more use had been got out of the thermometer. Meantime I endeavoured to estimate the probable thermal mass of the bulb by

carefully measuring it, and assuming a probable thickness of the glass. In dealing with problems of this sort it is necessary to express the specific heat in terms of the volume, and for this purpose the ordinary numbers which express the capacity for heat of unit weight have to be multiplied by the density, which expresses the weight of 1 c.c. of the substance. The density of mercury is 13·596, and that of ordinary glass is 2·45; their specific heats per unit weight are 0·033 and 0·19 respectively; whence the capacity for heat of 1 c.c. of mercury is 0·449, and of glass 0·466. If their specific heats are taken as identical and equal to 0·457, the error made will not be more than 2 per cent., in the extreme case where the bulb is all glass or all mercury.

Hence it appeared that there was no necessity for knowing the thickness of the glass of the bulb or the weight of mercury in it. For calorimetric purposes, a knowledge of the volume of the bulb suffices, and it is immaterial in what proportion the two substances are present. The figures on which this calculation are based are for ordinary soda or potash glass, which was no doubt used in the construction of the German thermometers which I was using.

Using the value 0·457 for the specific heat per unit volume of the bulb, and whirling the thermometer at the uniform rate of 6 m. per second, twelve observations were made of the thickness of the film of air heated to the full amount, corresponding to the fall of temperature of the thermometer. The difference between the initial temperature of the thermometer and that of the air varied from 18° C. to 2° C., and the resulting computed thicknesses of the film of air heated varied from 0·209 to 0·267 mm.; the mean value was 0·237 mm.

The measurement of the volume of the bulb requires some attention. The most convenient form of the bulb is the cylindrical, and it is also the most common. But the bulbs are very rarely truly cylindrical, they are often considerably tapered. It is not sufficient to measure the diameter of the bulb with callipers, it is necessary to measure the circumference at various parts of the bulb. One simple way is to envelop the bulb with a wrapper of tissue paper, like a cigarette, to blacken the edge of the paper which is laid inside. When the paper is neatly and smoothly laid on, pressure with the finger along the line of the inner edge of the paper produces a sharp impression of the edge on the overlapping paper. On unrolling the paper the exact envelope of the bulb lies between the blackened edge of the paper and the impression which it has made on the paper underlying it. The length of the bulb is very easily measured, and when the paper envelope has been, to begin with, given the proper length, it measures the outer surface of the bulb, less the surface of the end. This is assumed to be hemispherical, and is added accordingly. The upper end of the bulb, where the stem joins on, is neglected, as in

thermometers of German pattern, it takes little part in the exchange of heat with the outside. Another method of obtaining the exact circumference of the bulb, which is a little easier and perhaps more exact, is to wind fine thread round it, each turn touching its neighbour closely until, say, ten turns have been taken. The thread is then unwound and measured. The tenth part of the length is the circumference of the bulb. By measuring the axial space occupied by the ten turns, the correction for "pitch" can be ascertained, but if anything but very coarse thread is used it is negligible. The active superficial area of the bulb is given by adding to the hemispherical end surface the product of the mean circumference into the total length of the cylindrical part of the bulb. In like manner, the volume of the bulb is obtained by adding to the hemispherical volume of the end the product of the mean circular area into the length of the cylindrical part of the bulb. The volume, multiplied by 0·457, gives the thermally equivalent volume or weight of water.

Air thermometers of the simple kind described above, are very easily made so as to give calorimetrical results. It is only necessary to weigh and measure the piece of glass tube before blowing the bulb. The shortening of the straight part of the tube after blowing gives the length of it which has been expanded into a ball, and from the known length and weight of the original piece of tube, the weight of the bulb is found. By carefully gauging the volume of the ball its volume can be obtained, and from that the thickness of the glass. When the specific heat of the glass is known, the water value of the bulb is given; if the air contained is taken into account, the value is increased by from 1 to 2 per cent. The surface of the ball divided by

Table VI.—Particulars of Calorimetric Air Thermometers made of Lead Glass.

Number of Instrument.	1.	2.	3.	4.	5.
Original weight of tube (grm.)	17·724	18·508	18·4186	18·8136	18·6169
„ length of tube (mm.)	225·7	193·0	192·1	196·4	194·25
Ditto after blowing........	197·0	144·0	137·0	126·0	104·0
Difference................	28·7	49·0	55·1	70·4	90·25
Weight of 10 mm. tube (grm.)	0·7853	0·9590	0·9590	0·9580	0·9580
Weight of bulb (grm.).....	2·2538	4·6991	5·2841	6·7443	8·6550
Diameter of bulb (mm.)....	24	32	38	45	51
Volume of ditto (c.c.)......	7·238	17·157	28·731	47·713	69·456
Surface of bulb (sq. cm.)...	18·095	32·170	45·364	63·617	81·713
Volume of glass at sp. gr. = 3·0....................	0·7513	1·5664	1·7614	2·2481	2·8850
Thickness of glass (mm.)...	0·415	0·487	0·388	0·353	0·353
Water value of bulb, sp. heat = 0·57............	0·4282	0·8928	1·0040	1·2814	1·6445
Surface ÷ water value.....	42·26	36·03	45·18	37·25	42·24

the water value gives an expression for the sensitiveness of the instrument.

In Table VI the particulars of several air thermometers which I have had made are given. As they are made of lead glass, both the density and the capacity for heat are higher than in the case of German glass.

VII. "The Root of *Lyginodendron Oldhamium*, Will." By W. C. WILLIAMSON, LL.D., F.R.S., and D. H. SCOTT, M.A., Ph.D., F.L.S., F.G.S. Received March 14, 1894.

During a re-investigation of the structure of *Lyginodendron*,* the results of which we hope to lay before the Royal Society on a future occasion, an important fact has come to light, which we desire to place on record without delay.

A carboniferous fossil, with the structure perfectly preserved, has been described in previous memoirs, under the name of *Kaloxylon Hookeri*, Will.† We have now established the fact that *Kaloxylon* was not an independent plant, but was the root of *Lyginodendron Oldhamium.*

Specimens, presenting in every respect the typical *Kaloxylon* structure, have been found in actual continuity with the stem of *Lyginodendron*, arising from it as lateral appendages. Their structure and mode of origin prove that they were adventitious roots. These organs branched freely, and we have roots and rootlets of all sizes, and at all stages of development.

This discovery enables us to give a complete account of the vegetative organs of *Lyginodendron*, as we are now fully acquainted with the structure, not only of the stem and foliage, but also of the adventitious roots.

Presents, May 31, 1894.

Transactions.

London:—Camera Club. Journal. Vol. VIII. No. 96. 8vo. *London* 1894. The Club.

Entomological Society. Transactions. 1894. Part 1. 8vo. *London.* The Society.

Royal United Service Institution. Journal. Vol. XXXVIII. No. 195. 8vo. *London* 1894. The Institution.

* *Cf.* Williamson, "On the Organisation of the Fossil Plants of the Coal Measures," Part IV, 'Phil. Trans.,' 1873, p. 377; Part XVII, 'Phil. Trans.,' 1890, B., p. 89.

† *Cf.* "On the Organisation of the Fossil Plants of the Coal Measures," Part VII, 'Phil. Trans.,' 1876, Part 1, p. 1; Part XIII, 'Phil. Trans.,' 1887, B., p. 289.

NOTICES TO FELLOWS OF THE ROYAL SOCIETY.

The **Offices** and **Library** close at 4 P.M. (Saturdays 1 P.M.) during August and September.

By order of Council, no books can be given out during August, and all books borrowed are returnable by July 31.

The 'Proceedings' are sent by post to every Fellow of the Society who resides within the limits of the Postal Union. The 'Philosophical Transactions' and the 'List of Fellows' are delivered only to those Fellows who call for them, or who send a written application to the Assistant Secretary. Such an application may, if so desired, be filed as a standing order.

The separate Papers forming the 'Philosophical Transactions' will be sent post free, as issued, to those Fellows who prefer to have them in that form. And, on application to Messrs. Harrison and Sons, 46, St. Martin's Lane, these will be bound in a cloth case for 2*s.* 6*d.*, or the cloth cases for binding may be purchased, price 1*s.* 6*d.*

A printed post-card of the papers to be read at each meeting will be sent weekly to any Fellow upon application to Messrs. Harrison and Sons, 46, St. Martin's Lane, W.C.

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PROCEEDINGS OF

THE ROYAL SOCIETY.

VOL. LVI. No. 337.

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June 7, 1894.

June 14, 1894.

For continuation of Contents see 2nd and 3rd pages of Wrapper.

Price Six Shillings and Sixpence.

SEPTEMBER 21, 1894.

CONTENTS (*continued*).

Transactions (*continued*).

Munich:—K. B. Akademie der Wissenschaften. Sitzungsberichte (Math.-phys. Classe). 1894. Heft 1. 8vo. *München* 1894. The Academy.

New York:—American Museum of Natural History. Bulletin. Vol. VI. Pages 97—128. 8vo. [*New York*] 1894. The Museum.

Palermo:—Circolo Matematico. Rendiconti. Tomo VIII. Fasc. 1—3. 8vo. *Palermo* 1894. The Society.

Rome:—R. Comitato Geologico d'Italia. Bollettino. Anno 1894. No. 1. 8vo. *Roma*. The Committee.

Turin:—R. Accademia delle Scienze. Atti. Vol. XXIX. Disp. 5—10. 8vo. *Torino* 1894. The Academy.

Vienna:—Anthropologische Gesellschaft. Mittheilungen. Bd. XXIV. Heft 2. 4to. *Wien* 1894. The Society.

Observations and Reports.

Calcutta:—Meteorological Department, Government of India. Meteorological Observations recorded at Seven Stations in India. December, 1893. 4to; Monthly Weather Review. December, 1893. 4to. *Calcutta*. The Department.

London:—Army Medical Department. Report. 1892. 8vo. *London* 1894. The Department.

Potsdam:—Astrophysikalisches Observatorium. Publicationen. Bd. IX. 4to. *Potsdam* 1894. The Observatory.

Journals.

Archives Cliniques de Bordeaux. Année III. No. 4. 8vo. *Bordeaux* 1894. The Editors.

Burdett's Hospital and Charities Annual. 1894. 8vo. *London*. Mr. H. C. Burdett.

Firket (Ad.) Sur Quelques Roches Combustibles Belges assimilées ou assimilables au Cannel-coal Anglais. 8vo. *Liége* 1893; L'Origine et le Mode de Formation de la Houille. 8vo. *Liége* 1894. The Author.

Hinrichs (G.) Centenary Commemoration of Antoine-Laurent Lavoisier. 1794—1894. 4to. *St. Louis* 1894; Contributions to Atom-Mechanics, published in the Comptes Rendus of the Academy of Sciences of Paris, and in other serials. 8vo. *St. Louis* 1894. The Author.

Hyman (C. P.) An Account of the Coins, Coinages, and Currency of Australia. 8vo. *Sydney* 1893. The Author.

Reade (T. M.) A Cooling and Shrinking Globe and the Origin of Mountain Ranges. 8vo. *London* 1894; Continental Growth and Geological Periods. 8vo. *London* 1894. The Author.

June 7, 1894.

The Annual Meeting for the Election of Fellows was held this day.

The LORD KELVIN, D.C.L., LL.D., President, in the Chair.

The Statutes relating to the election of Fellows having been read, Sir Erasmus Ommanney and Mr. Scott were, with the consent of the Society, nominated Scrutators to assist the Secretaries in examining the lists.

The votes of the Fellows present were then collected, and the following candidates were declared duly elected into the Society :—

Bateson, William, M.A.
Boulenger, George Albert.
Bradford, John Rose, M.D.
Callendar, Professor Hugh Longbourne.
Cheyne, Professor William Watson, M.B., F.R.C.S.
Froude, Robert Edmund.
Hill, Professor M. J. M., M.A., D.Sc.
Jones, Professor John Viriamu, M.A., B.Sc.
Love, Augustus Edward Hough, M.A.
Lydekker, Richard, B.A.
Penrose, Francis Cranmer, M.A., F.R.A.S.
Scott, Dukinfield Henry, M.A., F.L.S.
Smith, Rev. Frederick John, M.A.
Swan, Joseph Wilson, M.A., F.I.C.
Veley, Victor Herbert, M.A., F.C.S.

Thanks were given to the Scrutators.

June 7, 1894.

The LORD KELVIN, D.C.L., LL.D., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the Newtonian Constant of Gravitation." By C. V. BOYS, F.R.S., A.R.S.M., Assistant Professor of Physics, Royal College of Science, South Kensington. Received May 31, 1894.

(Abstract.)

The Newtonian constant of gravitation G, *i.e.*, the force in dynes between 2 grams of matter 1 cm. apart, has been determined with a very accurately constructed piece of apparatus, designed on the lines which I laid down in my paper on the Cavendish experiment ('Roy. Soc. Proc.', vol. 46, p. 293). The important dimensions are approximately—

Distance between centres of lead balls *in plan* ..	6 in.
" " gold " ..	0·9 in.
Diameter of lead balls	$4\frac{1}{4}$ in.
" gold "	0·2 and 0·25 in.
Difference of level between right and left sides .	6 in.

The lead balls were hung by phosphor bronze wires from pillars in the lid of the apparatus, and the gold balls by quartz fibres from the ends of the "beam mirror." The beam mirror was supported by a quartz fibre, 17 in. from a torsion head. An elaborate system of screens protected the apparatus from temperature variations.

An "optical compass" of extreme precision was employed in measuring the horizontal distances between the fibres and between the wires, which alone among the geometrical magnitudes need be known with a very high degree of precision.

The scale was 9 ft. long, divided into 50ths of an inch. It was placed at a distance equal to 14,000 divisions. It could be read with certainty to 1/10 division. The deflections varied according to the circumstances of each experiment from 351 to 577 divisions, and the squares of the periods from 35,431 to 58,519 secs.^{2}

The experiments were carried out by permission of Professor Clifton, under the Clarendon Laboratory, at Oxford.

The result is for

G, the Newtonian constant of gravitation.... $6 \cdot 6576 \times 10^{-8}$
Δ, the mean density of the earth .... $5 \cdot 5270$.

II. "On the Recurrent Images following Visual Impressions." By SHELFORD BIDWELL, M.A., LL.B., F.R.S. Received March 27, 1894.

The earliest recorded observation which I have been able to find of a certain curious phenomenon associated with optical after-images is that of Professor C. A. Young, who published a note on the subject in the year 1872, and proposed that the phenomena should be called "recurrent vision."* He noticed that when a powerful Leyden jar discharge took place in a darkened room, any conspicuous object was seen twice at least, with an interval of a little less than a quarter of a second; often it was seen a third time and sometimes even a fourth. He thought that the phenomenon suggested the idea of a reflection of the nervous impulse at the nerve extremities, as if the intense impression upon the retina, after being the first time propagated to the brain, was reflected back to the retina and thence again to the brain, thus renewing the sensation of vision.

A few months later an account of two experiments on the same subject was published by Mr. A. S. Davis.† In the first, a piece of charcoal, one end of which was red-hot, was waved about so as to describe an ellipse or circle a few inches in diameter. A blue image of the burning end was seen following the charcoal at a short distance behind it, the space between the charcoal and its image being absolutely dark. The interval of time after which the sensation of blue light succeeded the primary sensation was estimated to be about a fifth of a second. The other experiment was made with a piece of apparatus resembling a photographic instantaneous shutter. The shutter was interposed between the observer's eye and the sky and was covered with pieces of coloured glass, through which momentary flashes of light were allowed to pass. It was found that each flash was, after a short interval, generally succeeded by a recurrent image, the colour of which was quite different from that of the glass. The results of Mr. Davis's observations are summarised below.

Mr. Davis remarks that except as regards the red glass, the recurrent colour does not differ much from the complementary colour,

* 'Phil. Mag.,' vol. 43 (1872), p. 343.

† *Ibid.*, vol. 44 (1872), p. 526.

Table of Mr. Davis's Observations.

Light transmitted.	Complementary colour.	Recurrent colour.
Deep blue............	Yellow	Greenish-yellow
Green	Blue-red...	Reddish-blue
Yellow...............	Blue	"
Orange-red...........	Blue-green............	Red-blue
Pure red.............	"	No image

and he concludes that when any one of the three kinds of Young-Helmholtz nerve fibres is excited, an excitation is induced in the nerve fibres of the other kinds, the process being analogous to the induction of electric currents.

In 1885 I called attention to a very simple and effective method of exhibiting a recurrent image.* If an ordinary vacuum tube, illuminated by an induction coil discharge, is made to rotate slowly upon a horizontal axis fixed at right angles to the middle of the tube, the tube is seen to be followed at a distance of a few degrees by a ghost-like image of itself, the ghost exactly imitating the original in form, but having a uniform steel-grey colour. In the same paper the following observation is noted :—" The vacuum tube being at rest in a feebly lighted room, I concentrated my gaze upon a certain small portion of it while the discharge was passing. The current was then interrupted and the luminous image was almost instantly replaced by a corresponding image which appeared to be intensely black upon a less dark back-ground. After a period which I estimated at from a quarter to half a second the black image again became luminous; this luminous impression lasted but for a small fraction of a second and the series of phenomena terminated with its disappearance . . . . . It was also found desirable to make the preliminary illumination as short as possible, a single flash being generally sufficient to produce the phenomena." The following comment was added :—" The series of phenomena seem to be due to an affection of the optic nerve which is of an oscillatory character. Abnormal darkness follows as a reaction after the luminosity, and again after abnormal darkness there is a rebound into feebler luminosity."

The subject has recently attracted much attention in connection with the experiments of M. Aug. Charpentier. The account of them given by M. Charpentier in a paper on "Retinal Oscillations"† is briefly as follows :—If a black disk having a white sector is illuminated by a strong light, and slowly turned round while the

* 'Nature,' vol. 32 (1885), p. 30.

† "Oscillations rétiniennes," 'Comptes Rendus,' vol. 113 (1891), p. 147. See also "Réaction oscillatoire de la Rétine," 'Arch. de Physiologie,' 1892, p. 541.

observer's eye is fixed upon its centre, there appears upon the white sector, near to its leading edge, a well-defined dark band, which is separated from the black ground of the disk by a similar white band. The angular extension of the dark band increases with the speed of rotation, so that it always takes the same time to pass over a fixed point on the retina; it begins about one-sixty-fifth or one-seventieth of a second after the first passage of the white, and lasts sensibly the same time. He goes on :—"The dark band is in fact only a kind of reaction of the retina after the luminous excitation, a reaction which can be demonstrated in a totally different manner. I have found that if an instantaneous luminous excitation is produced in complete darkness the sensation appears to be reduplicated; shortly after its first generation it seems to disappear and then manifest itself again. This is the case, for example, when a single discharge from a Ruhmkorff coil is passed through a Crookes or Geissler vacuum tube, or simply, but less obviously, through the air. . . . . There is, then, in this last experiment, as in the first, a negative reaction of the retina under the influence of excitation. . . . . It would be difficult, and in any case premature, to indicate the cause of this phenomenon, but it may fairly be characterised as the result of a retinal oscillation set up under the influence of the beginning of the luminous excitation." I think it clearly appears from the above extract that M. Charpentier was unacquainted with the earlier observations of myself and others.

In consequence of the importance which seemed to be attached by physiologists to the phenomena of visual reaction, as evidenced by Professor Burdon Sanderson's recent Presidential Address to the British Association,* I was induced to undertake the further experimental investigation, of which an account is given in the present paper. This deals partly with the colours of recurrent images under different conditions, and partly with the reaction attending the early stages of a luminous impression as noticed by Charpentier.

In the observation of the recurrent images set up by the action of light of different colours I began, like Mr. Davis, by using coloured glasses.

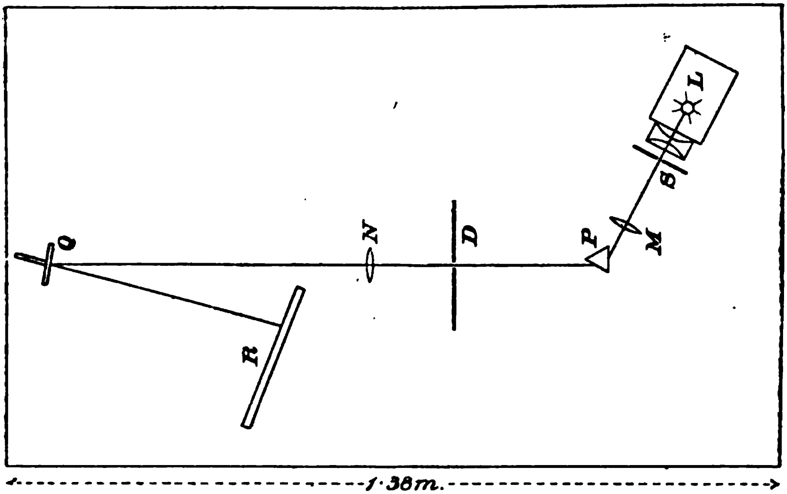
A metal disk, about 8 cm. in diameter, was arranged so as to rotate slowly and steadily about its centre in front of the condenser of a projection lantern. Near the edge of the disk was a circular aperture about 0·5 cm. in diameter, the image of which was focussed upon a distant screen. A plate of coloured glass was placed before the projecting lens, and thus was obtained a small, coloured disk of light, which described a circular path upon the screen. The coloured disk was, in most cases, seen to be followed at an interval of a few degrees by a ghost of the same size and shape, but of feebler luminosity, and of a hue which varied more or less with the colour of the glass

* 'Brit. Assoc. Rep.,' 1893. 'Nature,' vol. 48, p. 468.

employed. With white electric light the colour of the ghost was violet.

This method of experimenting was, however, found to be unsuited for the purpose in view, and I mention it only on account of the facility which it affords for exhibiting the phenomenon to a large number of persons. To obtain results of any value, it was necessary to employ the simple colours of the spectrum, and the arrangement finally adopted for this purpose is indicated in fig. 1. L is a lantern con-

FIG. 1.



taining a high-pressure oxyhydrogen light, which is better adapted for the experiment than an arc lamp, the intensity of the light being easily varied. S is an adjustable slit, M a projection lens, P a bisulphide of carbon prism, D a metal plate, in the middle of which is a circular aperture 2 mm. in diameter. A spectrum, 6 or 7 cm. in length, can be projected upon D, a small selected portion of it passing through the aperture and falling upon the mirror Q, which is 8 cm. in diameter. To the back of the mirror is attached a horizontal arm, which is not quite perpendicular to the mirror, its inclination being capable of adjustment. The arm is rotated by clock-work, and turns once in 1½ secs.

It was at first attempted to study the phenomenon by direct eye observations of the reflected image of the aperture in the rotating mirror, the aperture being covered by a piece of finely-ground glass; but, for pretty obvious reasons, no satisfactory results could be thus obtained.

A telescope was then employed, having a power of 12, and an eye-

piece with a large field. I believe that, after sufficient practice, this would be found the best possible method of observation; but it is exceedingly difficult to keep the eye absolutely steady, and untrained observers never succeeded in seeing the looked-for phenomena at all.* Since it seemed desirable that my own observations should be confirmed by others, I abandoned the telescope and the ground glass, and by means of the lens N focussed the reflected image of the aperture upon a white screen, R. The diameter of the projected disk of coloured light was about 1·5 cm., and that of the approximately circular path which it described, 30 cm. To aid in steadying the eye, a spot of luminous paint, upon which the gaze might be directed, was applied at the centre of the circle. With this arrangement, almost any one can see the ghosts without the smallest difficulty.

When the mirror turns once in 1½ secs., the ghost or recurrent image appears about 50° behind the coloured disk, the corresponding time interval being one-fifth of a second. Exact measurement is, however, not easy, and it is probable that the interval is not quite the same with light derived from different portions of the spectrum. The ghost appears to be circular in form, its diameter being generally rather less than that of the original. The colours of the recurrent images, as specified below, have all been observed by several persons, and, except as to those at the extreme limits of visibility, all the observations were in agreement.

Experiment 1.

Spectrum colours.	Recurrent colours.
Extreme violet.......	No perceptible image.
Middle violet	A pale image, variously described as grey, yellow, and greenish-yellow.
Dark blue...........	Feeble violet.
Light blue	Brighter violet.
Middle green	Bright violet. The image is more conspicuous with green light than with any other.
Greenish-yellow	Blue.
Orange-yellow	Bluish-green,
Orange	Dark bluish-green.
Orange-red..........	Very dark bluish-green.
Red................	No image at all, however bright the red was made.

The violets all appear to my own vision slightly redder than the violet of the spectrum.

The following experiment was then made.

* If a telescope is used, the mirror must be silvered on its outer surface, and in the air of a laboratory is quickly tarnished.

Experiment 2.

For the screen with the aperture at D, fig. 1, another was substituted, having a horizontal slit 7 cm. long and 2 mm. wide, the image of which was projected upon the screen R after reflection from the rotating mirror. Thus a small spectrum was produced, which revolved parallel to itself, in a circle about 1 metre in diameter.* The eyes were directed upon a fixed spot near one end of the horizontal diameter of the circle. The spectrum was followed by a ghost of the form rather roughly indicated in fig. 2. It extended from the orange of the spectrum to the beginning of the violet, terminating somewhat abruptly at the orange end, and fading away gradually at the other. The image was distorted, as shown in the figure, approaching nearest to the spectrum at about the middle of the green, a little on the more refrangible side of the most luminous portion. The distance separating the spectrum from the image increased more rapidly towards the red end of the spectrum than towards the violet end, and the image was widened out considerably at the violet end; but neither the moving spectrum itself nor its recurrent image was so sharply defined as appears in the diagram.

It was remarkable that the *whole* of the recurrent image of the spectrum was of a violet hue, being brightest where the distance from the spectrum was least. No trace whatever of yellow or greenish-yellow could be detected at the more refrangible end, nor of blue or bluish-green at the other.

The apparent absence of any colour except violet in the recurrent image of the complete spectrum is capable of two possible explanations. The greenish-yellow seen at one end, and the blue and bluish-green seen at the other, when the spectrum colours are tested separately, may be due merely to an effect of contrast, the true colour of the image being in both cases a weak violet. Or, on the other hand, these colours may really be present at the ends of the image of the whole spectrum, being, however, of too weak an intensity to be distinguishable when in proximity to the more luminous portions of the spectrum itself and of its image.

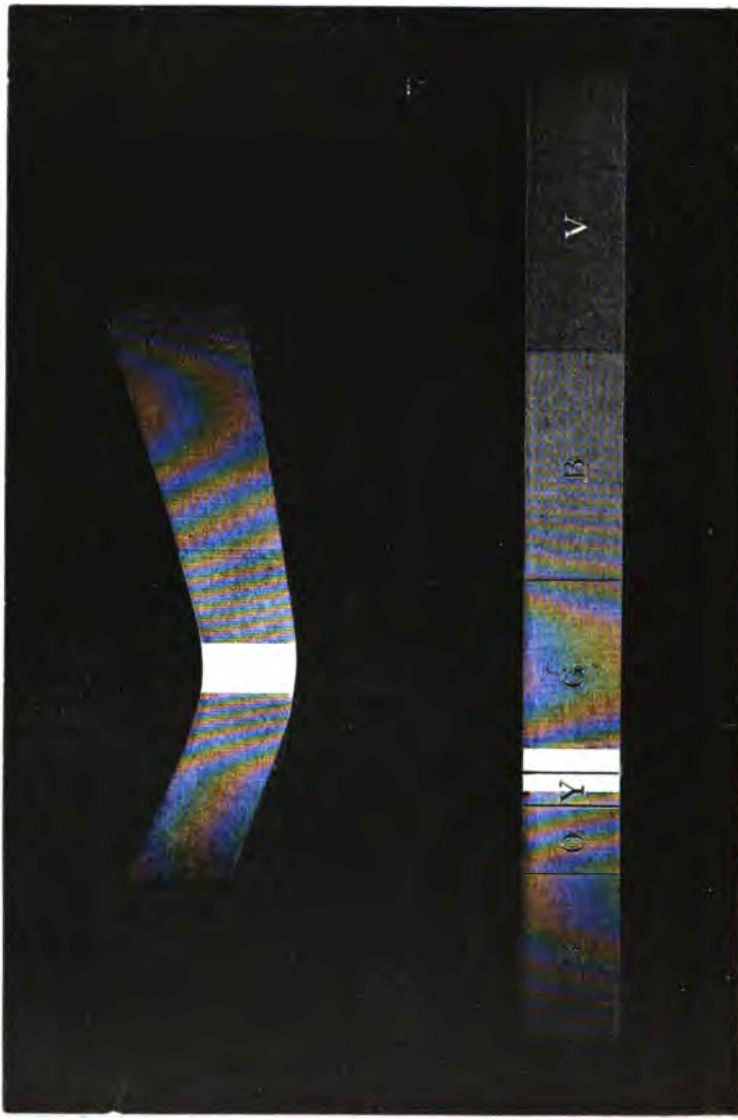
Two experiments were made in the hope of settling this question.

Experiment 3.

The slit at D was removed, and in its place was put a zinc plate having two small apertures close together. A second lantern and prism were set up, and two spectra were projected upon the zinc plate. By the help of screens, things were so arranged that a violet

* Helmholtz observed the after-images of a spectrum seen for an instant, but failed to notice the dark interval which preceded their appearance ('Phys. Opt.,' p. 376).

FIG. 2.



ray from one spectrum passed through one of the apertures, while a violet ray from the other spectrum passed through the other. These were reflected from the mirror (which was not rotated), and formed two violet disks side by side upon the screen. By adjusting the widths of the slits and the intensities of the limelights, one of

these disks was made as bright as possible, and the other very feeble. By the side of the bright violet disk the feeble one often seemed to be of a greenish-yellow hue, though, when seen alone, it was distinctly violet.

Experiment 4.

In a similar manner a feeble violet and brighter greenish-yellow were placed side by side, but, however much the intensity of the former was diminished, it could never be made to assume a blue colour at all comparable to that possessed by the recurrent image of the greenish-yellow. Nor did it appear bluish-green beside orange-yellow or orange-red.

While, therefore, the result of Experiment 3 is consistent with the contrast hypothesis, that of Experiment 4 appears to be opposed to it; but there is so great a difference in the circumstances of the two kinds of observation, the one involving a deliberate comparison of the colours of two stationary luminous disks, and the other an estimate formed while a disk and its recurrent image were in rapid motion, that the opposing evidence cannot be regarded as conclusive. Another experiment was therefore devised.

Experiment 5.

The original screen with one small aperture was placed at D, and two spectra were projected upon it in such a manner that a green ray from one spectrum, and a red ray from the other, passed through the aperture, forming red and green images which were exactly superposed upon the screen R. The colour of the single image thus formed could, by suitable regulation of the limelights, be made greenish-yellow, yellow, or orange-yellow, these colours being, of course, not simple ones, but compounds of red and green. Now, red by itself gives no recurrent image whatever (this was verified before proceeding further by shutting off the green ray), while green by itself gives a violet recurrent image. The question to be decided was whether the green, when accompanied by the inert red, would give a violet recurrent image as if it were alone, or whether the compound colour formed by the combination—greenish-yellow, for example—would be attended by a blue or bluish-green recurrent image, just as if the compound were a simple spectrum colour.

The latter was found to be the case. The same hue of greenish-yellow, whether a simple spectrum colour or a compound of red and green, was always attended by a blue ghost. When the red ray of the compound was shut off by a screen, the ghost instantly became violet: when the screen was removed it at once resumed its blue colour.

This experiment, though not conclusive, is clearly in favour of the probability that the blue and bluish-green recurrent colours apparently observed when the yellow and orange portions of the spectrum are tested separately are due merely to an effect of mental judgment, and not to any cause of a physiological nature.

There are, therefore, four independent facts which are consistent with the conclusion that luminous recurrent images are due to a reaction of the violet nerve fibres only.

(*a*.) With white light the recurrent colour is violet.

(*b*.) In the recurrent image of the complete spectrum no colour but violet can be detected.

(*c*.) A pure red light, however intense, gives no recurrent image. It is generally supposed by the supporters of the Young-Helmholtz theory that red light has no action upon the violet nerve-fibres.

(*d*.) The apparently blue colour of the ghost of simple spectrum yellow is just as well produced by a compound yellow consisting of green and red, the latter of which is inert when tested separately.

The path of the revolving spot of light is generally marked by a phosphorescent track, which, when the rate of revolution is not less than one turn in $1\frac{1}{2}$ secs., often forms a complete circle. The brilliancy of this luminous trail seems to vary with different observers, in some cases apparently being so intense that the recurrent image cannot be distinguished from it at all. The trail is due to the usually feeble continuation of the after-image, of which the bright initial stage constitutes the recurrent image.

A spot of red light, although it is never followed by a ghost, is always considerably elongated during its revolution, and its colour ceases to be uniform, the rear portion assuming a light bluish-pink tinge. However small the spot of light is made, and however high the speed of revolution, no complete separation of the spot into red and pink portions has ever been effected.

In the experiment next to be described the Charpentier effect and the recurrent image are made to exhibit themselves simultaneously.

Experiment 6.

Two blackened zinc disks, 15 cm. in diameter, from each of which two opposite quadrants were cut out,* were mounted in contact with each other on a horizontal axis, driven by clockwork and making one turn in $1\frac{1}{2}$ secs. By slipping the disks over one another round their

* It was found necessary to cut out two quadrants instead of only one, in order to balance the disks and secure uniform rotation.

centres, opposite open sectors might be obtained, of any aperture from 0° to 90°. The apparatus was set up opposite a box containing a 32-candle power incandescent lamp, with a variable resistance in the circuit, the side of the box between the lamp and the disks being covered with a sheet of ground glass.

The sectors being in the first place opened as widely as possible, I fixed my eye upon the centre of the double disk, and at once saw Charpentier's dark band upon the illuminated background.

The sectors were then gradually closed up, until the posterior edge of the dark band approximately coincided with that of the sector.* When this was accomplished it was found that the arc of the open sector was equal to about $\frac{1}{52}$ part of the whole circumference. The dark reaction, therefore, ceased in ($\frac{1}{52}$ of $1\frac{1}{2}$ secs. =) $\frac{1}{35}$ sec. after the first impact of the light upon the eye.

For more readily demonstrating the succeeding phenomena, it was found convenient to again open the sectors a little, so that they covered an angle of about 10° or 12°. Resuming the observation, it was seen that the posterior edge of the open sector was bordered by a luminous fringe due to persistence. A little beyond the termination of the fringe there appeared an intensely black radial band, estimated to cover a space of from 3° to 4°, and easily visible even upon the black ground of the metal disk, though it is shown far more conspicuously upon a translucent disk made of stout writing-paper, with a sector cut out. Lastly, after another interval of, perhaps, 35° or 40°, came the luminous recurrent image,† which, with the yellowish light of the incandescent lamp, appeared to be of a blue colour. By varying the angular aperture of the sector, it was ascertained that the recurrent image appeared at a fixed interval after the light was cut off, and not after its first impact.

This method of observation revealed one other point of interest, which seems hitherto to have escaped notice, though it is evident enough with a Charpentier disk, when once attention has been directed to it. The average illumination of the bright band intervening between the dark band and the leading edge of the sector is much more intense than that of the other portion of the sector. Moreover, it is not uniform, but increases, gradually at first, and very rapidly at last, from the leading edge up to the dark band. In fact when the light used is not strong, the luminous margin of the bright band is a far more conspicuous object than the dark band itself: it appears to glow almost like a white-hot wire.

Charpentier states that, under favourable conditions, he has been

* This was not a very easy operation, because the luminous sector was slightly widened by persistence, especially near the circumference. indistinct ?

† This, of course, cannot be seen upon a translucent paper disk being over-powered by the transmitted light.

able to detect the existence of a second, and even of a third, dark band of greatly diminished intensity, though he adds that the observation is a very difficult one.* What is probably the same effect in a different form can, however, be shown quite easily in the following manner.

Experiment 7.

In a blackened zinc disk 15 cm. in diameter, there were cut two opposite radial slits, about 0·5 mm. in width. The disk was rotated at the rate of one turn per second in front of a sheet of ground glass, behind which was an incandescent lamp. The glass was covered with opaque paper, in which a circular opening was made of slightly less diameter than the disk. The disk was placed opposite this opening, and no light reached the eye except such as passed through the two slits. When the disk was observed from a distance of about $1\frac{1}{2}$ metres, the eye being fixed upon its centre, each slit appeared to give four (or possibly five) luminous images, arranged like the ribs of a partly opened fan. The images were distinctly separated by dark intervals near the circumference, but overlapped one another towards the centre. The leading image was naturally the brightest, each consecutive image being considerably weaker than its precursor. All had the same tone of colour, namely that of the yellowish-light given by the electric lamp. The usual blue recurrent image could also be seen following the images of the radial slits, at an angle of about 80°.

It appears, then, that when the retina is exposed to the action of light for a limited time, the complete order of visual phenomena is as follows:—

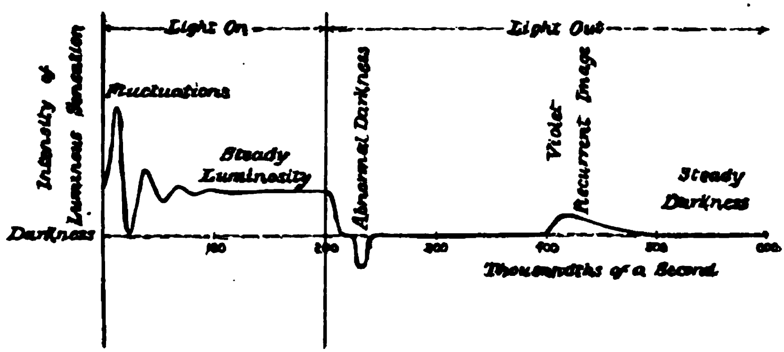
(1) Immediately upon the impact of the light there is experienced a sensation of luminosity, the intensity of which increases for about one-sixtieth of a second: more rapidly towards the end of that period than at first.

(2) Then ensues a sudden reaction, lasting also for about one-sixtieth of a second, in virtue of which the retina becomes partially insensible to renewed or continued luminous impressions. These two effects may be repeated in a diminished degree, as often as three or four times.

(3) The stage of fluctuation is succeeded by a sensation of steady luminosity, the intensity of which is, however, considerably below the mean of that experienced during the first one-sixtieth of a second.

* I have noticed that the intensity of the dark band always appears to fluctuate very rapidly, perhaps twenty or thirty times in a second. The rate of fluctuation is quite regular, and independent of the rate of rotation.

FIG. 3.



(4) After the external light has been shut off, a sensation of diminishing luminosity continues for a short time, and is succeeded by a brief interval of darkness.

(5) Then follows a sudden and clearly-defined sensation of what may be called abnormal darkness—darker than common darkness—which lasts for about one-sixtieth of a second, and is followed by another interval of ordinary darkness.

(6) Finally, in about a fifth of a second after the extinction of the external light, there occurs another transient impression of luminosity, generally violet coloured, after which the uniformity of the darkness remains undisturbed.

An attempt has been made in fig. 3 to give a rough diagrammatic representation of the above-described chain of sensations. No account has been taken of the comparatively feeble after-image, to which the phosphorescent trail before referred to is due, and which may last for two seconds or more.

In conclusion, it may not be unnecessary to add a warning that, though all the effects here described have been witnessed without much difficulty by several persons besides myself, it is hardly probable that any one, who is quite unaccustomed to observations of the kind, will be entirely successful in a first attempt at repeating the experiments.

Addendum. May 24th.

Since the above was written, there has been brought to my notice an important paper by Dr. Carl Hess, "On the After-images following luminous Impressions of short Duration."* In his principal experiments momentary illumination was produced by means of an instantaneous shutter, giving an exposure of 1/100 or 1/200 second.

* Pflüger's 'Archiv für Physiologie,' vol. 49 (1891), p. 190.

Observations were made of the effects following excitation by white light, by coloured light derived from different portions of the spectrum, and by the whole spectrum at once. In all his experiments, Dr. Hess noticed the occurrence of what he speaks of as a negative after-image of very short duration (corresponding to what I have called an interval of darkness) which followed almost immediately upon the termination of the illumination, and preceded what is "commonly known as the positive after-image." He states that this negative after-image which, according to his measurements, lasted for about one-third or one-half second, was overlooked by Helmholtz, Aubert, Fich and others. Hering, however, had reasons for suspecting its existence, and it was to test this point that the experiments in which Hering himself co-operated, were undertaken.

The negative after-images are stated not to have been represented in all cases by mere darkness. Under favourable conditions, the "dark after-image" succeeding a momentary excitation by coloured light, was tinted with a colour complementary to the original one; and when the stimulus was generated by the complete spectrum, all the complementary colours were seen for a short time after its disappearance. No such complementary tints have ever revealed themselves in my own experiments, the space between the primary luminous image and its ghost always appearing as simply dark.

The colours assigned by Dr. Hess to the "positive after-images" also differ from those observed by myself. In most cases he describes the positive after-image as either having a feeble colour of the same hue as that of the light employed for the stimulus, or as being colourless.

Dr. Hess considers, as I do, that the brightest portion of the positive after-image of the spectrum corresponds with the green, and remarks that the brightness decreases gradually towards the more refrangible end of the spectrum, and much more quickly towards the less refrangible end.

Such discrepancies as seem to exist between Dr. Hess's results and my own may perhaps be accounted for by the very different methods of observation which we employed. A stationary stimulus would, no doubt, be better adapted than a moving one for developing the feeble tints of the dark negative after-images, as well as those exhibited by the bright positive after-images during by far the greater part of their continuance, which, according to Dr. Hess's estimate, is as long as from four to eight seconds. On the other hand, the method adopted by myself discloses the important fact, of which Dr. Hess makes no mention whatever, that the positive images are immensely brighter for a very brief initial period—not more than one-tenth of a second—than during their subsequent existence. While this phase of transient brilliancy altogether failed to attract Dr. Hess's notice,

it constituted in my own experiments the chief and most striking phenomenon: and it was to the colours which appeared during the bright phase that my attention was exclusively directed, the tints of the relatively insignificant "luminous trails" being too faint to be distinguishable.

It is clear that the momentary excessive brightness of the positive image is no less essential than the dark interval (or negative after-image) for the generation of the phenomenon of recurrent vision which forms the subject of the present paper.

III. "Niagara Falls as a Chronometer of Geological Time." By J. W. Spencer, Ph.D. Communicated by Professor T. G. Bonney, F.R.S. Received March 16, 1894.

(Abstract.)

1. *Conjectures as to the Age of Niagara Falls.*—Prior to the writing of the present paper, most of the conjectures as to the age of the Falls have been based simply upon the supposed uniform rate of recession. Thus, in 1790, Andrew Ellicott assigned 55,000 years as the age of the Falls. In 1841, Sir Charles Lyell allowed 35,000 years; in 1886, Professor R. S. Woodward, after three surveys had been made, calculated the age as 12,000 years; and later, Mr. G. K. Gilbert, supposing the recession to progress at the maximum axial retreat alone, reduced the age of the Falls 6,000 years. This latter was not intended as an estimate, as he fully recognised that such a time must have been greatly lengthened by many changing conditions. The rate adopted by the first two writers was only conjectural, as no surveys had then been made. Three surveys had been completed before the writings of the latter two writers, and I have had the benefit of a fourth. Woodward's calculation was upon the mean mathematical enlargement of the Horseshoe gulf at the end of the chasm, which rate was less than the geological rate of retreat. The author's method differs from the others in that it takes into consideration the rate of recession throughout the changing episodes of the river, which have been entirely discovered by Gilbert or himself. His computations make the age surprisingly near to the conjecture of Lyell.

2. *Modern Topography.*—This section of the paper gives such details as bear upon the subject, some of which do not appear elsewhere.

3. *Geology of the District.*—Besides what may be found in other works, there are several measured sections and descriptions showing the amount of work the river had to do. Several figures illustrate the varying conditions.

4. *Ancient Topography.*—The Niagara is a modern river. It crosses a broad ancient valley nearly 100 ft. deep, in the vicinity of the Falls. This depression has largely escaped the attention of even geologists, and entirely in its bearing upon the history of the Falls. The peculiar extension of the chasm at the Whirlpool, and the buried valley of St. David's, have been considered by many as part of a preglacial Niagara river. This is now found to be a branch of a buried valley outside the Niagara *cañon*, and hundreds of feet shallower, with ancient sloping V-shaped walls, whilst those of the gorge are vertical. It is only an incident that the modern river touched this drift-filled valley, but it has given rise to the elongation of the chasm at the Whirlpool. The drainage of the tableland in ancient times was across the direction of the Niagara river, and was strongly marked by bold limestone ridges, which have only been penetrated by the Falls in modern times. Even the Erie basin emptied by a route several miles west of the Niagara.

5. *Basement of the River.*—In order to explain the work done by the river, this feature is described, part of the banks of the original course, before sinking into the chasm, being on hard rocks, and part on local deposits of drift. Even the deserted river banks carved out of such accumulations are still well preserved.

6. *Discharge of the Niagara River.*—This is only important in order to learn what is the discharge of the Erie basin alone; for during a considerable portion of the life of the Niagara only the Erie waters fell over the falls. The drainage of the Erie basin is 3/11 of that of the four great upper lakes.

7. *Modern Recession of the Falls.*—From four surveys, extending over a period of forty-eight years, the mean modern rate of recession of the Falls is found to be 4·175 ft. a year. Its rate is variable with secular episodes of rapid medial recession, followed by its cessation along the axis, but with increased lateral retreat. This cycle appears to take about fifty years. But the detailed figures are given with a map. This rate is, however, excessive, on account of the geological conditions favouring the rapid modern recession, but the rate taken for the mean recession under the conditions of the modern descent of the river with the present discharge is 3·75 ft. a year.

8. *Sketch of the Lake History and the Nativity of Niagara River.*—At one time a great proportion of the lake region was covered by a single sheet, or the Warren Water. Upon its dismemberment—in part, at least, by the rise of the land—one large lake was formed occupying the basins of Huron, Michigan, and Superior; and another a portion of the Erie extending into the Ontario basin. The waters in these two basins were subsequently lowered, so that they fell to their rocky eastern rims, and the three upper lakes discharged by way of Lake Nipissing and the Ottawa river, and the Niagara had its birth,

draining only the Erie basin. Then the Niagara river descended 200 ft. In course of time the waters subsided 220 ft. more, but eventually they were raised again 80 ft. at the mouth of the Niagara, thus reducing the descent of the river, from the head of the rapids above the falls to the foot of the last rapids in its course to the lake, to 320 ft. During the lowest stage, Ontario lake receded twelve miles from the end of Niagara gorge, where the falls had been located at their nativity.

9. *Laws of Erosion.*—Theoretically the erosion varies as the height of the falls and the volume of the water, but some of the work is converted into heat. The recession is largely due to the work being expended in the undermining of the hard capping rocks, by the removal of the underlying shales. The rate of the modern recession has been determined under the changing conditions of erosion, so that the theoretical variations of other portions of the river's work includes their modification.

10 and 11. *Episodes of the River and the amount of Recession in each. Duration of each Episode.*—First episode: Water falling 200 ft., in volume 3/11 of modern discharge; gorge, 11,000 ft. long; duration, 17,200 years. Second episode: river descending 420 ft., in three cascades; first stage, only the discharge of the Erie waters; length of chasm, 3,000 ft.; duration, 6,000 years; second stage, drainage of all the upper lake; length of chasm, 7,000 ft.; duration, 4,000 years. Third episode: same volume and descent as in last, but the three falls united into one fall; length of chasm, 4,000 feet; duration, 800 years. Fourth episode: volume of water as at present, the level of lower lake as to-day; first stage, a local rapid making the descent of 365 ft.; work particularly hard; length of gorge, 5,500 ft.; duration about 1,500 years; the second stage as at present; work easy; length of *cañon*, 6,000 feet; descent of water, 320 ft.; rate of recession here taken as the full measured amount of 4·175 ft. a year; duration, 1,500 years. Thus the age of the falls is computed to be 31,000 years, with another 1,000 years as the age of the river before the nativity of the Falls. The turning of the Huron waters into the Niagara was about 8,000 years ago. A difficult question was the amount of work done in each episode. This was in part determined by the position of the remaining terraces corresponding to different stages of the river, and by the changing effects of erosion.

12. *Relations between the Terrestrial or Epeirogenic Movements and the Falls.*—The deserted beaches in the lake region have been deformed by unequal terrestrial elevation, and this movement has caused the changing conditions of the river in a large part, such as the turning of the Huron waters from the Ottawa valley to the Erie basin. This deformation affecting the Niagara district, since the commencement of the river epoch, amounts to 2·5 ft. per mile; east of Lake Huron,

4 ft. per mile; and at the outlet of Lake Ontario, 5 ft. per mile; all in a north-eastward direction. Taking the amount of movement in each district as representing also the proportional measure of time, then calculations can be made upon several of the beaches, and in terms of the age of Niagara their antiquity can be inferred. The importance of the computations in this paper is that they support the correctness of the calculated age of the Falls. In the application of these results it appears that the rate of terrestrial uplift in the Niagara district is about 1·25 ft. a century; 2 ft. east of Lake Huron, and 2·5 ft. at the outlet of Lake Ontario. Here was found the first long looked-for indication of the rate of uplift.

13. *The Relation of Niagara Falls to Geological Time.*—From the study of the deserted beaches, it appears that the commencement of the lake epoch was as long before the birth of Niagara Falls as the Falls are old, so that the beginning of the lake age was probably 64,000 years ago, or perhaps even 80,000 years. Against this conjecture we have as yet no proof. On the other hand, some suppose the lakes to have been held in by glacial dams, continuing for long episodes at the same level, and by the withdrawal of the glaciers the waters were lowered in addition to the terrestrial deformation. With this assumption, the retreating ice continued until the end of the Iroquois episode, or from our computations until 14,000 years ago. But here we need much more investigation. The present paper is merely a contribution in a field of work in America, in which only a few workers have so far contributed the detailed labours upon which this study is built.

14. *The End of the Falls.*—From the rate of terrestrial elevation and the rate of recession of the Falls, it appears that if the movements continue as they have been progressing, then before the Falls shall have retreated to Lake Erie, the Niagara outlet will have been deserted, and the waters of the upper lakes will discharge by way of Chicago into the Mississippi drainage, a change analogous to the turning of the Huron waters into the Erie valley from the Ottawa outlet. This change might be expected 7,000—8,000 years hence.

IV. "The Influence of Intra-Venous Injection of Sugar on the Gases of the Blood." By VAUGHAN HARLEY, M.D., Teacher of Chemical Pathology, University College, London, Grocer Research Scholar. Communicated by GEORGE HARLEY, M.D., F.R.S. Received May 9, 1894.

In a paper on "The Effects and Chemical Changes of Sugar injected into a Vein"* I showed that when grape sugar is injected

* 'Roy. Soc. Proc.,' 1893.

into the jugular vein of a dog it causes an augmentation in the quantity of lactic acid in the circulation, the quantity of the acid steadily increasing until it reaches its maximum in about three hours after the injection. It then gradually, hour by hour, decreases, until in about six hours it returns to the normal amount. The question as to the base with which the lactic acid combines to form a lactate is, however, still unsettled.

The results of Walter's* experiments, in conjunction with the often-noticed fact that ammonia is increased in the urine of diabetes, led me to imagine that the lactic acid combined with ammonia, until I found that the breaking up of sugar in the organism has no influence whatsoever on the amount of ammonia in the blood, and consequently it cannot be the base.

It then appeared to me probable that the lactic acid had combined with the bases of carbonates in the blood, having driven out the carbonic acid from its compound.

In order to try and settle this point, I estimated the quantity of carbonic acid in the blood under different conditions.

The series of experiments I am now about to record were performed in the Physiological Institute at Leipzig, and I wish to express my gratitude to Professor C. Ludwig for the kind assistance he gave me in the matter.

The experiments, which were made on dogs, were conducted in the same manner as in my previous researches, above alluded to, except that blood was withdrawn only three times from each dog. In order to obtain a normal standard, the first specimen of blood was taken before the sugar was injected, the second was withdrawn in an hour, and the third in from three to five hours after the intra-venous injection of the sugar.

In order that the composition of the blood might be altered as little as possible by the bleeding, only 30 c.c. of blood was collected each time.

In all cases the blood was collected under mercury from the carotid artery. The gases were pumped from the blood by means of a Ludwig mercurial pump, and analysed by Bunsen's method.

The quantities of gases found were calculated at 0° C. and 760 mm. of mercury, and are expressed in volumes per cent.

Before alluding to the changes found in the blood gases, I will briefly give, in a tabular form, the results obtained from each experiment:—

* Walter, 'Arch. Exper. Path. u. Pharm.,' vol. 7, p. 158, 1877.

Experiment 1.

Weight of animal.	Condition.	Quantity injected of			Volumes per cent. at 0° C. and 760 mm. Hg.	
		Sugar, in grams.		NaCl solution.	Carbonic acid.	Oxygen.
kilos.		total.	per kilo.	c.c.		
7	Before sugar injection ...	60	8·56	120	37·380	22·280
	1 hour after ..	..	..	..	27·006	17·071
	5 hours after..	..	..	..	34·357	14·886

The only nerve symptoms after the sugar injection were manifested in vomiting and muscular tremors. These were not accompanied by coma or any other symptom.

The quantity of carbonic acid found in the standard specimen of blood was 37·380 per cent., whereas, in that taken an hour after the sugar injection it was only 27·006 per cent., that is to say, a diminution of 10·374 per cent. in the amount of carbonic acid followed upon the intra-venous injection of the sugar, while the blood withdrawn five hours later contained 34·357 per cent. of carbonic acid, this being only 3·023 per cent. less than that in the standard blood. The carbonic acid, thus tending to return to the normal amount, showed that the influence of the sugar on the carbonic acid in the blood is merely temporary.

The quantity of oxygen in the standard blood was found to be 22·280 per cent. An hour after the sugar injection it had fallen to 17·071 per cent., thus giving a diminution of 5·209 per cent. Five hours later it was still further decreased, being only 14·886 per cent. Consequently, in this respect the effect of the sugar on the oxygen is different from that upon the carbonic acid.

Experiment 2.

Weight of animal.	Condition.	Quantity injected of			Volumes per cent. in 0° C. and 760 mm. Hg.	
		Sugar, in grams.		NaCl solution.	Carbonic acid.	Oxygen.
kilos.		total.	per kilo.	c.c.		
5	Before sugar injection ...	50	10	100	38·541	19·902
	1 hour after ..	..	..	..	28·042	7·220
	4½ hours after.	..	..	..	28·926	13·968

The nerve symptoms following the injection of the sugar were greater in this case. The vomiting and tremors of the limbs were followed by well-marked epileptic fits, which, an hour later, were succeeded by a semi-comatose condition. Although the animal could still be roused, it remained in a sleepy condition up to the third bleeding, when it was killed.

The percentage of carbonic acid fell in the first hour after the sugar injection, while the animal was in a drowsy condition, from 38·541 to 28·042. This gives a diminution of carbonic acid of 10·499 per cent. Four and a half hours after the injection the carbonic acid had risen to 28·926 per cent. That is to say, it was 9·615 per cent. less than the quantity found in the standard blood.

The oxygen which originally stood at 19·902 per cent. fell, in the first hour, to 7·220 per cent.; therefore it was 12·682 per cent. less than the normal amount. In four and a half hours after the sugar injection it increased to 13·968 per cent., which is only 5·934 per cent. less than the original quantity found.

Thus it appears in this case there was a greater diminution in both the carbonic acid and oxygen of the blood during the first hour than in the former experiment; a result corresponding with the far greater nerve disturbances, and no doubt due, as stated in my former paper, to a larger percentage of sugar to bodily weight having been injected into the circulation. It was found in this case that the carbonic acid was, four and a half hours after the injection of the sugar, while the animal was in a semi-comatose state, almost as low as during the first hour. The oxygen had by this time, on the other hand, markedly increased in quantity.

These results having been obtained, it was decided to withdraw the third portion of blood somewhat earlier after the sugar injection than in the foregoing cases.

Experiment 3.

Weight of animal.	Condition.	Quantity injected of			Volumes per cent. at 0° C. and 760 mm. Hg.	
		Sugar, in grams.		NaCl solution.	Carbonic acid.	Oxygen.
kilos.		total.	per kilo.	c.c.		
23	Before sugar injection. ..	240	10·435	480	42·260	
	1 hour after ..	..	..	..	33·075	10·217
	3 hours after..	..	..	..	33·000	14·569

Here the nervous symptoms which showed themselves in the form of vomiting, trembling, and two epileptic attacks were followed by

drowsiness, which lasted until after the second bleeding. The drowsiness in this case passed off before the third bleeding.

In the standard specimen of blood the carbonic acid was 42·260 per cent., and an hour after the intra-venous injection of the sugar it fell to 33·075 per cent., being a decrease of 9·185 per cent. The third portion of blood taken in three hours, that is to say after the drowsiness had passed off, was found to contain 38·000 per cent. of carbonic acid, a decrease from the normal of 4·260 per cent.

The specimen of oxygen from the normal blood was lost. The quantity found an hour after was 10·217 per cent., and three hours after the sugar injection it had increased to 14·569 per cent.

The results of this experiment, as far as they go, correspond very closely with those of Experiment 1; in which there was likewise only a very slight nervous disturbance.

Experiment 4.

Weight of animal.	Condition.	Quantity injected of			Volumes per cent. at 0° C. and 760 mm. Hg.	
		Sugar, in grams.		NaCl solution.	Carbonic acid.	Oxygen.
kilos.		total.	per kilo.	c.c.		
20·5	Before sugar injection....	230	11.2	460	39·520	16·025
	1 hour after ..	..	..	..	32·140	15·561
	3 hours after..	..	..	..	24·725	17·767

Although this dog had vomiting and marked tremor of the limbs there were no epileptic seizures. Sleepiness, however, came on later, and was marked at the time of the third bleeding.

In the first specimen of blood the quantity of carbonic acid was 39·520 per cent., and it diminished during the first hour after the sugar injection to 32·140 per cent., being a decrease of 7·380 per cent. Three hours after the injection of the sugar, the carbonic acid fell still further, it being then only 27·725 per cent., that is to say 14·795 per cent. less than the original amount.

The oxygen, which at the beginning was 16·025 per cent., decreased during the first hour to 15·561 per cent., being a loss of 0·464 per cent. By the third hour it again rose to 17·767 per cent., that being 1·742 per cent. more than was found in the normal blood.

As in Experiment 2, this dog had become semi-comatose by the third bleeding, the carbonic acid being then even less than what it was during the first hour.

Having now briefly given the results met with in each separate experiment, I will now consider the results as a whole.

In the first place, we see there was a decrease in the quantity of carbonic acid in all the different specimens of blood during the first hour after the sugar was injected, the diminutions being 10·374, 10·499, 9·185, and 7·380 per cent.

In the second place, the blood, taken five hours after the sugar was injected, showed a decrease of 3·023 and 9·615 per cent. (in Experiments 1 and 2); while after three hours there was a decrease of 4·260 per cent. (in Experiment 3). In all the three cases it had therefore shown, during the later hours, a more or less marked tendency to return to the normal amount. In Experiment 4 the blood at the third hour contained 14·795 per cent. less carbonic acid than the normal blood, and 4·415 per cent. less than what it contained at the first hour. This discrepancy may be due to the fact that a greater percentage of sugar was injected, and the dog was in consequence rendered more comatose. This view seems the more likely, as in Experiment 2, when the dog was semi-comatose, the carbonic acid was markedly diminished at the fifth hour.*

These united results support the view that the lactic acid derived from the splitting up of the sugar in the animal body drives off the carbonic acid from the sodium salts and replaces it. This view is still further supported by the fact that the quantity of carbonic acid in the blood withdrawn at the different periods after the sugar injection, varied in the same manner as the quantity of lactic acid had been found to do. In both cases during the first hour after the sugar injection, one finds larger quantities than during the later hours.

Whether the percentage decrease in the amount of the carbonic acid hinders its elimination by the lungs or not will depend upon how much power the combined lactic acid has of hindering the blood from taking up the carbonic acid from the tissues and the tension of the existing gas.

This point would be ascertained by estimating the quantity of carbonic acid expired after the sugar injection. The experiments I have already published† on this point show that there is no decrease in the amount of carbonic acid expired from an animal immediately after sugar has been injected into its circulation. In fact there was an actual increase of carbonic acid in all but one case‡ during the first hour.

* "Small dogs," as I stated in my former paper, "are relatively much more susceptible than large ones to the effect of sugar injection."

† Vaughan Harley, "Influence of Sugar in the Circulation on the Respiratory Gases," 'Journal of Physiol.,' vol. 15, p. 139, 1893.

‡ *Ibid.*, Exp. 9, p. 147.

There was a marked decrease of carbonic acid during the later hours in those cases which suffered from coma.

It would thus seem that in those cases when there are no nervous symptoms caused by the intra-venous injection of sugar, while the quantity of lactic acid is at its highest and the quantity of carbonic acid in the blood is at its lowest, more carbonic acid is expired than before the injection of the sugar.

An explanation to this fact, if it really exists, is at the present moment impossible. In order to settle this point it would be necessary in the same animal to make all the analyses at the same time; which would be impossible without an exceptionally large dog, as the quantity of blood needed would cause of itself changes in the metabolism.

In the next place the changes met with in the quantity of oxygen in the blood are still more surprising, as there is no known reason why hæmoglobin should not take up the usual amount of oxygen as it does in health after the intra-venous injection of sugar.

In all of the experiments the quantity of oxygen is seen to have been markedly diminished during the first hour after the sugar injection.

In three of them it fell 0·464, 3·209, and 12·682 per cent. below the normal standard during the first hour. This result can be partially explained by the influence of the endosmotic flow of the juices of the tissues into the circulating blood, which is known to occur when the quantity of sugar in the blood is increased. For in a series of similar experiments Brasol* found that during the first five minutes after the injection of sugar the proteids of the serum were reduced to even below one half of their previous amount, while one or two hours after the injection the proteids had, as a rule, returned to the normal amount.

During the third and fifth hours after the sugar injection it will be noticed that the quantity of oxygen in the arterial blood was 14·886, 13·968, 14·569, and 17·767 per cent.; that is to say the quantity that is usually found in venous blood.

The diminution in the quantity of oxygen, even from three to five hours after the sugar injection, cannot therefore be explained on a dilution theory.

* Brasol. Du Bois-Reymond's 'Archiv.,' 1884.

V. "Contributions to the Life-History of the Foraminifera." By J. J. LISTER, M.A., St. John's College, Cambridge. Communicated by Professor ALFRED NEWTON, F.R.S. Received May 7, 1894.

(Abstract.)

The phenomenon of dimorphism is now known to be presented by many different species of Foraminifera.

The individuals of a species fall into two groups. In one the central chamber (the *Megasphere* of Munier-Chalmas and Schlumberger) is of considerable size, while in the other it is small (*Microsphere*). These two forms of a species may be distinguished as the *Megalospheric* and *Microspheric* forms.

They have been shown to differ, not only in the size of the central chamber, but, in some instances (Miliolidæ), in the plan on which the chambers are arranged, in the size attained by the full-grown shell, and also in the frequency of their occurrence, the megalospheric form being much the more abundant.

It has been suggested that the different conditions under which *Orbulina universa* is found represent the megalospheric and microspheric forms, but the reasons urged in favour of this view appear inconclusive.

Polystomella crispa (*Linn.*).

With the hope of throwing light on the life history of the Foraminifera, a large number of specimens of this species have been examined.

Like so many others, it is dimorphic. Though the two forms are indistinguishable when the shell is complete, on examining decalcified and stained specimens they may be at once referred to one form or the other. The central chamber of the megalospheric form is generally about 80μ in diameter, while that of the microspheric form is about 10μ. Associated with the difference in the size of the central chambers there is a marked difference in the nuclei of the two forms. The relative frequency of the megalospheric form to the microspheric in 1812 examples, is as 34 to 1.

In the *Microspheric* form numbers of small nuclei are present, scattered through the protoplasm, but not extending into the terminal chambers. Those in the inner chamber are smaller than those situated further on. The nuclei contain nucleoli of different sizes lying in an apparently homogeneous internucleolar substance. It is shown that the nuclei increase in number by simple division, and it appears probable that they are so derived from a single nucleus.

After maintaining their rounded form for a certain time, the nuclei give off portions of their substance into the surrounding protoplasm. This process appears to begin in the innermost chambers, but it extends to the nuclei in the outermost chambers, and ultimately the whole of the nuclear material is distributed through the protoplasm in the form, in preserved specimens, of irregularly branched and deeply staining strands. Of the further history of the microspheric form I have no clear evidence.

The *Megalospheric* form during the vegetative period of its life has a single large nucleus, which grows in size with the growth of the protoplasm, and passes on from chamber to chamber, moving towards the centre of the protoplasm contained in the series of chambers, though lagging some distance short of it. It consists of a nuclear reticulum, nucleoli which occupy the nodes of the reticulum, and of a substance occupying the meshes. The nucleoli appear to increase in number and diminish in size with the advance of the organism. There is reason to believe that as the nucleus moves on through the chambers portions of its substance are given off into the protoplasm. It appears that this may occur either by the separation of considerable portions, sometimes containing several nucleoli, which lie strewn along the track of the nucleus, or by the dispersal of minute fragments into the surrounding protoplasm, causing in stained specimens a flush in the neighbourhood of the nucleus. In some specimens the nucleus has lost its rounded form, and sends irregular processes into the protoplasm. Its staining properties are at the same time diminished. It appears probable that these nuclei are such as have given off a large part of their substances as above described, and are now in process of dissolution.

In the reproductive phase no large nucleus is present, but hosts of minute nuclei (1—2 μ in diameter) are found scattered through the protoplasm. At the same time broad channels of communication have become opened up, setting the inner chambers in direct communication with the outer.

At first the small nuclei are most abundant in the terminal chambers, but ultimately they become uniformly distributed through the protoplasm. They then divide by karyokinesis, the protoplasm being aggregated about them in spherical masses, 3·5 μ in diameter, each of which contains a dividing nucleus.

At a later stage each nucleus, presumably the daughter-nuclei of this division, becomes the centre of a flagellated spore. These spores are all of approximately equal size, in other words, they are *isospores*.

In one instance spores of a different character were observed escaping. These were *anisospores*. They consisted of *macrospores*, globular bodies having a diameter of 11—10 μ, and with indications of a flagellum, and *microspores* of a globular or oval shape, from

6—1 μ in diameter and provided with two flagella, one longer than the other, rising close together from the body of the spore. I am unable to say whether the parent of these spores was megalospheric or microspheric, but as the isospores are produced by the individuals of the former type it is possible that the anisospores belong to those of the latter.

Orbitolites complanata, Lamk.

In the *Microspheric* form, the centre of the disc is occupied by small chambers. Numbers of rounded nuclei are distributed through the protoplasm, often in pairs, and in some cases they may be seen to be united by a constricted band, as though in process of simple division. Larger solitary nuclei with a well marked reticulum are also present.

In the later stages of growth large brood chambers are formed at the periphery of the disc, which Brady found to be crowded with young ("primitive discs") of the megalospheric form. Examination of specimens preserved in spirit in which the young are present in the brood chambers, shows that the inner part of the shell is empty, its contents being represented only by the young. A large nucleus is present in the "primordial chambers" of the young discs.

The centre of the *Megalospheric* form is occupied by the "primitive disc." This consists of a large "primordial chamber" (the megalosphere), which is usually pyriform, and measures about 100μ in length, surrounded by the very large "circumambient chamber." The small chambers of the remainder of the disc are arranged about the primitive disc in rings.

The nucleus which, as has been said, occupies the primordial chamber in the young form, maintains that position during a large part of the growth of the shell. Ultimately it appears to break up into irregular fragments, which become dispersed through the adjoining chambers.

The specimens of this form from Celebes have all attained a larger size than those from Tonga and Fiji. In three cases (out of 114) the protoplasm has left the central region of the disc, and is massed in brood chambers at the periphery in the form of megalospheric young, exactly resembling in shape and size those borne by the microspheric form. It is thus established that both the megalospheric and microspheric forms of *Orbitolites* under certain circumstances, produce young of the megalospheric type.

An examination of specimens of *Rotalia beccarii* (*Linn.*), *Truncatulina lobatula*, *Walker and Jacob*, *Calcarina hispida*, *Brady*, and *Cycloclypeus* has furnished evidence of the relation of nuclear characters to the two forms of a species analogous to that obtained in *Polystomella*.

Summary and Conclusions.

The following statements relating to the life-history of the Foraminifera appear to be justified :—

1. The species are in a great number of cases dimorphic. The dimorphism has been stated to exist in twenty-three genera, belonging to four out of the ten families into which Brady divided the group.

2. The two forms differ from one another—

(a) In the size of the central chamber. Their difference in this respect is in many cases very marked but may be slight (*Truncatulina*).

(b) In the shape and mode of growth of the chambers succeeding the megalosphere and microsphere.

(c) In the character of their nuclei. In this paper it is shown that in several species the microspheric form has many comparatively small nuclei, while the megalospheric form has a single large nucleus.

3. The megalospheric form of a species is much more numerous than the microspheric.

4. The megalospheric form has been seen to arise in some cases (at least seven genera) as a young individual already invested by a shell, produced in the terminal or peripheral chambers of the parent. While in some cases (*Orbitolites*) the parent of such megalospheric young was microspheric, in others (*Peneroplis*, *Orbitolites*) it was megalospheric.

5. Foraminifera, in certain conditions, give rise to active swarm cells.

These have been previously recorded in *Gromia* and *Cymbalopora*. In *Polystomella* the protoplasm of a megalospheric form was found broken up into swarm cells of uniform size (*isospores*), and similar bodies in a flagellated condition have been seen escaping.

The production of *anisospores* has been recorded in *Miliola* (Schneider), and it occurs also in *Polystomella* as stated above.

The question has arisen: are the two forms of the Foraminifera distinct from their origin, or is one a modification of the other? The following reasons may be urged for rejecting the latter hypothesis :—

Among the Miliolidæ the plan of growth is often entirely different in the two forms. The hypothesis of modification would in this case require a remodelling of the whole shell.

If such modification were to occur, various stages in the replacement of the megalosphere by small chambers should be found. So far as I am aware such stages have not been found.

While the megalospheric form is not found in process of transition into the microspheric, it is found, either with the protoplasm broken up into swarm cells (*Polystomella*), or containing megalospheric young in the peripheral chambers, while the central chambers are empty (*Orbitolites*). In both cases the megalosphere remained unabsorbed at the centre of the shell.

The microspheric form is found in the young condition.

The nuclear characters of the two forms are, at any rate, in the species which I have examined, quite distinct.

It appears then that it may safely be concluded that the microspheric and megalospheric forms are distinct from their origin.

What then is their relationship?

When two forms of a species are met with in animals or plants they generally either represent different sexes, or they are members of a recurring cycle of generations.

The hypothesis that the two forms of the Foraminifera represent the two sexes appears to be disproved by the fact that in *Orbitolites complanata*, both megalospheric and microspheric forms are found with the young of the megalospheric form (primitive discs) in their brood chambers. Other genera furnish analogous, though less complete evidence. Hence it is impossible to regard either form as male.

We turn then to the other hypothesis that the two forms are members of a recurring cycle of generations. On this view it is necessary to suppose, from the evidence afforded by *Orbitolites complanata*, in which both microspheric and megalospheric forms have been found with the young of the megalospheric form in their brood chambers, that the megalospheric form may, at any rate in some genera, be repeated for one or more generations, before the microspheric form recurs. No evidence of such a repetition has, however, been furnished by the examination of *Polystomella*.

The view that the life-history of the Foraminifera comprises more than one generation is in harmony with the fact that the nuclear history of the two forms in Polystomella, so far as it has been observed, presents resemblances to that which Brandt has recently described in *Thalassicola* among the Radiolaria. In this group, as is well known, the individuals of a species fall into two sets, those producing isospores and those producing anisospores, which are regarded as an asexual generation alternating with a sexual.

The simultaneous division of nuclei by karyokinesis immediately before the formation of the reproductive elements which was observed in the megalospheric form of *Polystomella* is a phenomenon of very general occurrence. A similar division has been shown to occur

in several genera of the *Mycetozoa* immediately before the formation of the spores, and it appears probable that the phenomenon is akin to the division of the micro-nucleus which precedes conjugation in the *Infusoria*, and to the division of nuclei which occurs in the maturation of the reproductive elements in the higher forms of animals and plants.

Presents, June 7, 1894.

Transactions.

Buda-Pesth:—Ungarische Geologische Gesellschaft. Földtani Közlöny. Kötet XXIV. Füzet 1—5. 8vo. *Budapest* 1894. The Society.

Cracow:—Académie des Sciences. Bulletin International. Avril, 1894. 8vo. *Cracovie.* The Academy.

Kharkoff:—Société des Sciences Expérimentales. Travaux de la Section Médicale. 1893. [*Russian.*] 8vo. *Kharkoff* 1894. The Society.

Lausanne:—Société Vaudoise des Sciences Naturelles. Bulletin. Vol. XXX. No. 114. 8vo. *Lausanne* 1894. The Society.

Leipsic:—Königl. Sächs. Gesellschaft der Wissenschaften. Abhandlungen (Math.-phys. Classe). Bd. XXI. No. 1. 8vo. *Leipzig* 1894; Berichte über die Verhandlungen (Math.-phys. Classe). 1894. No. 1. 8vo. *Leipzig* 1894. The Society.

London:—British Astronomical Association. Journal. Vol. IV. No. 6. 8vo. *London* 1894. The Association.

Geologists' Association. Proceedings. Vol. XIII. No. 7. 8vo. *London* 1894. The Association.

Photographic Society of Great Britain. Journal and Transactions. Vol. XVIII. No. 9. 8vo. *London* 1894. The Society.

New York:—American Museum of Natural History. Bulletin. Vol. VI. Pages 139—160. 8vo. [*New York*] 1894. The Museum.

Paris:—École Normale Supérieure. Annales Scientifiques. Tome XI. No. 5. 4to. *Paris* 1894. The School.

Société de Géographie. Bulletin. Tome XIV. Trim. 4. 8vo. *Paris* 1894. The Society.

Société Mathématique. Bulletin. Tome XXII. No. 4. 8vo. *Paris* [1894]. The Society.

Santiago:—Sociedad Nacional de Mineria. Boletin. Año XI. No. 65. 4to. *Santiago de Chile* 1894. The Society.

Journals.

Archives Néerlandaises des Sciences Exactes et Naturelles. Tome XXVII. Livr. 4—5. Tome XXVIII. Livr. 1. 8vo. *Harlem* 1894. Société Hollandaise des Sciences, Harlem.

Boletin de Minas Industria y Construcciones. Año X. Num. 2—3. 4to. *Lima* 1894. Escuela Especial de Ingenieros, Lima.

Giurisprudenza Internazionale. Anno I. Fasc. 1—12. 8vo. *Napoli* 1893. The Editor.

Alcock (A.). Natural History Notes from H.M. Indian Marine Survey Steamer "Investigator." Series II. No. 1. On the Results of the Deep-Sea Dredging during the Season 1890–91 (*cont.*). 8vo. [*London*] 1894. The Author.

Caruel (T.). Epitome Florae Europae terrarumque affinum. Fasc. 2. 8vo. *Florentiae* 1894. The Author.

Charleton (A. G.). The Choice of Coarse and Fine-crushing Machinery and Processes of Ore Treatment. 7 Parts. 8vo. *Newcastle-upon-Tyne* 1892–94. The Author.

Collins (F. H.). Twelve Charts of the Tidal Streams on the West Coast of Scotland. Folio. *London* 1894. The Author.

Espin (Rev. T. E.). The Distribution of Stars of Type III and of Stellar Spectra in Space. 8vo. *Crook* [1894]. The Author.

Galilei (Galileo). Opere: Edizione Nazionale. Vol. IV. 4to. *Firenze* 1894. R. Ministero della Istruzione Pubblica, Rome.

Helmholtz (H. von), For. Mem. R.S. Über den Ursprung der richtigen Deutung unserer Sinneseindrücke. 8vo. *Hamburg* [1894]. The Author.

Hull (E.), F.R.S. Artesian Boring at New Lodge, near Windsor Forest (Berks.). 8vo. [*London*] 1894. The Author.

Laws (F. A.). On an Apparatus for the Measurement of Coefficients of Self-induction, and the Investigation of the Phenomena of Alternating Currents. 8vo. [*Boston* 1894]; Preliminary Note on a Method for the Harmonic Analysis of Alternating Currents. 8vo. [*Boston* 1894]. The Author.

Meyer (O. E.). Ueber electrische Eisenbahnen. 8vo. *Breslau* [1894]. The Author.

Meyer (O. E.) und K. Mützel. Ueber die Störungen des Fernsprechverkehrs durch electrische Strassenbahnen. 8vo. *Berlin* 1894. The Authors.

June 14, 1894.

The LORD KELVIN, D.C.L., LL.D., President, in the Chair.

Mr. William Bateson, Mr. George Albert Boulenger, Professor Hugh Longbourne Callendar, Professor William Watson Cheyne, Mr. Robert Edmund Froude, Mr. Augustus Edward Hough Love, Mr. Francis Cranmer Penrose, Dr. Dukinfield Henry Scott, the Rev. Frederick John Smith, Mr. Joseph Wilson Swan, and Mr. Victor Herbert Veley were admitted into the Society.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "The Molecular Surface-energy of the Esters, showing its Variation with Chemical Constitution." By Professor W. RAMSAY, Ph.D., F.R.S., and Miss EMILY ASTON, B.Sc. Received April 26, 1894.

The investigation of the thermal relations of a series of esters by Professor Young* has made it possible to determine their molecular surface-energies between ordinary temperature and their critical points; for the two important constants required for the calculation of these properties, the densities of the liquids and of their vapours in the saturated state (their orthobaric volumes) have been carefully determined by him. Professor Young has had the kindness to place his specimens at our disposal; their purity is sufficiently guaranteed by the proofs afforded in his paper. Before using them they were tested for acidity, to ensure that no hydrolysis had occurred during accidental exposure; but in no case was the reaction acid.

The chief question to which an answer was sought was: Do these bodies confirm the general law of which experimental proof was furnished by one of the authors in conjunction with Dr. Shields, which may be thus stated—

At approximately equal intervals of temperature below their critical temperatures all normal liquids possess equal molecular surface-energy?

The analogy of this law with that of Boyle is very striking; the latter may be expressed in almost identical terms—

* 'Trans. Chem. Soc.,' vol. 63, p. 1191.

At equal intervals of temperature above absolute zero all normal gases possess equal molecular volume-energy.

By "molecular volume-energy" is understood the product of pressure into molecular volume, that is, into the volume occupied by the molecular weight of the gas taken in grams; while molecular surface-energy signifies the product of surface-tension and molecular-surface, that is, the surface on which equal numbers of molecules are supposed to be uniformly distributed, equal to the two-thirds power of the molecular volume of the liquid.

The apparatus employed for low temperatures was that figured in the 'Transactions of the Chemical Society,' vol. 63, p. 1094. A double set of observations was made, each set with a different capillary tube. One of the tubes was accidentally broken during the experiments, and was replaced by one of approximately the same radius. The radii, as described in a previous paper, were measured by means of a microscope with micrometer eye-piece; tube A had a radius of 0·01843 mm.; tube B of 0·01708 mm.; and tube C of 0·01046 mm. These measurements were confirmed by determining the ascent of pure benzene in the tubes at known temperatures, and this is, on the whole, the easiest and most accurate method of determining their diameters.

For higher temperatures, the apparatus, described in the 'Philosophical Transactions,' 1893, A, p. 662, was employed. In order to apply a correction for the capillary rise in the barometer-tube in which the capillary tube D was confined, a determination was made with each ester at some temperature (usually the boiling point of alcohol under atmospheric pressure) at which capillary rise had been determined in a wide tube with tube A, B, or C, where correction was unnecessary, the ascent being taken, as customary, in inverse proportion to the radii of the tubes. As the variation of capillary rise with temperature is approximately a linear one, a sufficiently accurate correction may be obtained by assuming a rectilinear relation. Thus, for example, if at 78° the rise in the wide tube was 30 mm., and in the narrow tube 27 mm., it was necessary to add 3 mm. to the rise in the narrow tube at that temperature. At the critical temperature the correction is, of course, zero, since at that temperature there is no capillary ascent in any tube. It was held that this difference decreased in the barometer tube proportionately with rise of temperature, so that, for example, if the critical temperature were 278°, at the temperature 178° the correction applied amounted to 1·5 mm.

We regarded it as unnecessary to increase labour by taking observations at each 10° rise of temperature, since a few points on the curve serve to show whether the rectilinear relation holds. The plan of experiment was as follows:—The pressure tube containing the capillary tube was heated in the vapour of chlorobenzene, boiling under atmospheric pressure about 132°, the exact temperature

naturally depending on the barometric pressure of the day. As none of the esters boiled much above 100° at atmospheric pressure, it was possible by lowering pressure to cause them to boil at 132°, especially as in filling the tube a trace of air was purposely left in the liquid. It is not necessary that this air should be visible as a bubble, but it is sufficient if the liquid is not thoroughly boiled *in vacuo*. It may appear strange that such a course was followed, but repeated experience has shown that if a liquid is wholly deprived of dissolved gas by boiling it *in vacuo*, it is impossible to cause it to boil, even at atmospheric pressure, although heated to 100° above its normal boiling point.

Having determined the capillary rise at 132°, the pressure in the jacket was lowered, so as to cause the chlorobenzene to boil at 78° or 80°, care being taken not to allow the gas present in the upper part of the tube to condense wholly. The rise was again noted. The tube was then jacketed with quinoline vapour at about 185°, as well as at higher temperatures, and readings were again taken. Some six or seven points on the curve were thus determined, a sufficient number to characterise it.

It will conduce to clearness to give the essential data at this stage, reserving details of experiment to the Appendix, where they are tabulated. For completeness' sake, the results previously published in the 'Philosophical Transactions' for methyl formate and for ethyl acetate are here included.

As the molecular surface-energy of a liquid, provided it does not dissociate with rise of temperature, may be calculated by means of the equation

$$\gamma(Mv)^{\frac{2}{3}} = k(\tau - d)$$

(where k is a constant characteristic of each liquid but varying only slightly from 2·1, τ is the temperature measured from the critical point downwards, and d is a constant), the liquid is sufficiently characterised by giving the values of k, the critical temperature, and d. They are as follows :—

Table I.

Ester.	Critical temperature, C.°	k.	d.
Methyl formate	214·0	2·042	5·9
Methyl acetate	233·7	2·109	4·5
Methyl propionate	257·4	2·182	5·3
Methyl butyrate	281·25	2·220	3·75
Methyl isobutyrate....	267·55	2·248	5·25

Table I—*continued.*

Ester.	Critical temperature. C.°	k.	d.
Ethyl formate........	235·4	2·020	4·5
Ethyl acetate.........	251·0	2·226	6·7
Ethyl propionate......	272·9	2·240	4·9
Propyl formate	264·85	2·110	4·85
Propyl acetate........	276·2	2·227	5·0

It is evident at the first glance that it is the acid radical which determines the value of k, for it increases progressively with the progressive increase of its molecular weight. Young has noticed a similar relation to hold with the ratios of absolute temperatures at corresponding pressures to absolute critical temperatures, but in other relations which he has investigated, there does not appear to be any analogous regularity.

As regards the values of d, they appear to fluctuate as the series is ascended in the order of complexity of acid radical, but too great dependence must not be placed on the values given. A very small change in k would make a considerable difference in the value to be assigned to d.

An attempt has been made to ascertain whether molecular volumes admit of more regular comparison at temperatures at which molecular surface-energies are equal. This appears, however, not to be the case. Thus, at the value 390 ergs, the group of four isomerides gives the following numbers :—

Ester.	Molecular volume.
Methyl butyrate	126·17
Methyl isobutyrate............	125·93
Ethyl propionate..............	126·80
Propyl acetate	127·16

The agreement is no better than at their boiling points under normal pressure.

An attempt has also been made to find whether the boiling points at corresponding pressures bear a constant ratio to the temperatures of equal molecular surface-energy. Taking that ratio of pressure to critical pressure given in the fifth line of the table on p. 1245 of the paper in the 'Transactions,' and dividing the corresponding temperature for each liquid by the temperature of equal molecular surface-energy, the following ratios are obtained :—

		Ratio.
Methyl formate..........	295·6/280·1	= 1·055
Ethyl formate...........	311·4/310·9	= 1·002
Methyl acetate..........	313·75/317·5	= 0·9879
Propyl formate..........	332·5/348·2	= 0·9545
Ethyl acetate............	327·7/342·1	= 0·9625
Methyl propionate.......	331·1/346·3	= 0·9559
Propyl acetate...........	346·75/369·2	= 0·9393
Ethyl propionate........	344·6/367·0	= 0·9391
Methyl butyrate.........	348·3/375·0	= 0·9290
Methyl isobutyrate.......	338·75/362·0	= 0·9356

These numbers may be roughly arranged into four groups: methyl formate, the isomerides of the formula $C_3H_6O_2$, those of the formula $C_4H_8O_2$, and those of the formula $C_5H_{10}O_2$. They suffice to show that the molecular surface-energies are not comparable for non-isomeric bodies at corresponding pressures.

It must, therefore, be concluded that, although a certain rough analogy exists between the corresponding temperatures and pressures of the esters and their molecular surface-energy, yet the causes which determine deviation from the deductions from the equations of condition for fluids, are still more operative in causing deviations when surface forces are under consideration.

These experiments add eight more compounds to the list of six given in the 'Phil. Trans.,' 1893, A, p. 662, showing that within wide limits of temperature the molecular surface-energy of non-associating compounds is a linear function of the temperature; and as the law has been found to hold between more restricted limits of temperature for other thirty ('Trans. Chem. Soc.', vol. 63, p. 1191), it may be taken as placed on a firm basis.

A certain number of substances, among which are to be found the alcohols and the acids, show deviation from this law. Reserving to another occasion the grounds for inferring that this deviation points to molecular complexity, it is advisable to inquire here whether it is legitimate to assume for compounds which follow the law that their molecular weight in the state of liquid is the same as that of their gases.

Strictly speaking, the conclusion does not follow. The similar form of the surface-energy equation to that expressing volume-energy is a mere analogy; there is no physical connexion as yet manifest between the two.* There is no positive evidence to show that the molecules of such liquids as follow the law do not associate in twos, or threes, on assuming the liquid state. But one thing is certain, if they do, all associate to an equal extent, and the degree of association is not altered by rise of temperature.

* This conclusion must be modified in view of the recent memoir by van der Waals (see p. 181).

These two assertions are probably not true; it is unlikely that mere liquefaction should produce in all cases equal association; and it is unlikely that a rise of temperature should not cause the dissociation of an associated body. Change from the gaseous to the liquid state may be regarded as essentially equivalent to increase of pressure, since each produces approach between the molecules, diminishing intermolecular distance, and bringing so-called chemical forces into play. Now it is well known that equal rise of pressure does not always produce equal increment of association; hence it is unlikely that association to an equal extent should be caused by the reduction of the volumes of compounds until they are approximately equal.

This kind of proof is not unknown to chemists; it is employed, tacitly perhaps, in the fundamental statement that the molecular formulæ of hydrogen, oxygen, nitrogen, &c., are H_2, O_2, and N_2. On this basis rests the usually accepted molecular formulæ of all compounds, and they are accepted because they are the simplest expressions which admit of equations of chemical interchange being written. It is true that subsequently to the adoption of such a standard its justice was confirmed by Kundt and Warburg's determination of the ratio between the specific heats of mercury gas at constant pressure and at constant volume, thereby rendering it extremely probable that the molecular formula of mercury is Hg_1, and consequently that of hydrogen H_2; and by the discovery by Victor Meyer that the molecular formula of iodine at high temperatures corresponds with I_1. But such confirmations merely supported the generally received assumption (for assumption it was then) that the molecular formulæ of most gases are directly comparable with that of hydrogen as H_2.

Even at this present date the doctrine of the uniform expansion of gases at high temperatures rests on a similar basis. It has been shown by Victor Meyer that at the highest temperature attainable in a gas-furnace—some 1700°—hydrogen, oxygen, and nitrogen maintain the same ratio of expansion towards each other. One of two conclusions follows:—either that the expansion of all three gases is uniform with increase of temperature, or that all three gases dissociate equally with equal rise of temperature. Needless to say that the first alternative is universally adopted.

We have thought it well to state in full the reasons for adopting the assumption that the molecular weights of such liquids as the esters are not changed on their assuming the liquid state. It is now evident that such a statement is an assumption, a hypothesis; but it is one for which there is a great deal of probability, probability of the same kind as that which led to the adoption of the usually received molecular formulæ for gases.

Appendix.

I. Experimental Results at Low Temperatures.

Substance.	Radius = 0·01843 cm.				Radius = 0·01046 cm.			
	t.	h.	γ.	$\gamma(Mv)^{\frac{2}{3}}$.	t.	h.	γ.	$\gamma(Mv)^{\frac{2}{3}}$.
Ethyl formate	10·0°	2·851	24·08	443·5	10·0°	5·051	24·22	446 0
Crit. temp., 235·30°	46·5	2·430	19·50	371·5	46·5	4·327	19·71	375·5
	78·5	2·072	15·68	309·2	78·5	3·650	15·68	309·1
Methyl acetate	10·0°	2·970	25·22	462·8	10·0°	5·200	25·06	459·9
Crit. temp., 233·7°	46·2	2·502	20·32	383·9	46·2	4·442	20·49	387·2
	78·3	2·121	16·28	318·2	78·3	3·752	16·35	319·5
Propyl formate	10·0°	3·017	25·02	523·6	10·0°	5·325	25·06	524·4
Crit. temp., 264·85°	46·2	2·612	20·67	446·3	46·2	4·611	20·71	447·1
	78·2	2·300	17·52	387·0	78·2	4·033	17·44	385·2
Methyl propionate	10·0°	3·248	25·23	524·3	10·0°	5·364	25·51	530·2
Crit. temp., 257·4°	46·2	2·816	20·85	447·3	46·2	4·629	20·98	450·2
	78·2	2·430	17·11	378·8	78·2	4·002	17·26	381·9
		Radius	= 0·01708	cm.				
Propyl acetate	10·0°	3·291	24·80	580·2	10·0°	5·392	24·88	582·0
Crit. temp., 276·2°	46·2	2·896	20·86	503·0	46·2	4·723	20·84	502·2
	78·2	2·518	17·35	431·0	78·2	4·126	17·41	432·3
		Radius	= 0·01708	cm.				

I. Experimental Results at Low Temperatures—*contiuued.*

Substance.	Radius = 0·01708 cm.				Radius = 0·01046 cm.			
	t.	h.	γ.	$\gamma(Mv)^{\frac{2}{3}}$.	t.	h.	γ.	$\gamma(Mv)^{\frac{2}{3}}$.
Ethyl propionate	10·0°	3·253	24·57	574·0	10·0°	5·335	24·67	576·2
Crit. temp., 272·9°	46·2	2·856	20·58	496·1	46·2	4·672	20·62	496·9
	78·2	2·501	17·24	428·1	78·2	4·082	17·22	427·8
Methyl butyrate*	10·0°	3·115	25·63	595·0	10·0°	5·460	25·50	591·7
Crit. temp., 281·25°	46·2	2·736	21·50	514·5	46·2	4·795	21·39	511·8
	78·2	2·410	18·15	446·9	78·2	4·223	18·05	444·4
Methyl isobutyrate	10·0°	3·195	24·11	563·6	10·0°	5·213	24·08	563·0
Crit. temp., 267·55°	46·2	2·806	20·29	487·3	46·2	4·572	20·04	486·1
	78·2	2·426	16·70	415·0	78·2	3·964	16·64	415·1

* The tube used had the radius 0·01843 cm.

II. Experimental Results at Higher Temperatures.

Radius of Tube = 0·011197 cm.

Substance.	t.	h.	γ.	$\gamma(Mv)^{\frac{2}{3}}$.	$\gamma(Mv)^{\frac{2}{3}}$. Calc.	k and d.
Methyl formate.....	..	..	..	..	..	2·042 d = 5·9°
Ethyl formate	80·0°	3·381	15·50	306·2	304·9	2·020 d = 4·5°
	131·6	2·344	9·51	201·0	198·8	
	185·0	1·224	3·86	91·5	92·7	
	210·0	0·685	1·75	44·7	42·2	
Methyl acetate......	78·2°	3·498	16·31	318·9	318·5	2·109 d = 4·5°
	132·4	2·392	9·81	206·0	204·2	
	185·0	1·202	3·90	91·0	93·2	
	200·0	0·872	2·51	61·3	61·6	
	215·0	0·512	1·21	31·5	30·0	
Propyl formate	85·0°	3·665	16·60	371·9	369·2	2·110 d = 4·85°
	131·7	2·796	11·53	272·5	270·7	
	185·0	1·740	6·14	157·2	158·2	
	210·0	1·251	3·86	104·2	105·4	
	237·0	0·659	1·68	49·2	48·5	
Ethyl acetate	..	..	..	..	..	2·226 d = 6·7°
Methyl propionate ..	78·0°	3·750	17·31	383·1	379·3	2·182 d = 5·3°
	132·6	2·682	11·09	261·8	260·7	
	184·9	1·634	5·73	147·0	146·7	
	237·7	0·500	1·14	34·3	31·4	
Read from curve .. {	240·0	0·440	0·96	29·3	26·4	
	250·0	0·188	0·31	10·1	4·4	
Propyl acetate......	100·0°	3·474	15·10	382·8	381·3	2·227 d = 5·0°
	132·6	2·877	11·78	310·2	308·7	
	185·0	1·893	6·76	191·5	192·0	
	210·0	1·412	4·57	135·5	136·3	
	238·2	0·854	2·30	73·5	73·5	
Ethyl propionate....	100·0°	3·442	14·97	380·1	376·2	2·240 d = 4·9°
	132·2	2·880	11·77	310·2	304·1	
	185·0	1·864	6·59	187·5	185·9	
	210·0	1·384	4·41	131·5	129·9	
	237·6	0·812	2·14	68·8	68·1	
Methyl butyrate	100·0°	3·591	15·92	400·2	394·1	2·220 d = 3·75°
	132·5	3·001	12·49	325·3	321·9	
	185·0	2·033	7·41	207·3	205·3	
	210·0	1·532	5·09	148·7	149·8	
	238·0	0·980	2·81	87·7	87·7	
Methyl isobutyrate ..	100·0°	3·311	14·36	365·0	364·8	2·248 d = 5·25°
	132·2	2·728	11·10	292·8	292·3	
	185·0	1·726	6·03	172·1	173·8	
	210·0	1·250	3·90	117·0	117·6	
	237·6	0·694	1·73	56·6	55·5	

II. "The Complexity and the Dissociation of the Molecules of Liquids." By Professor W. Ramsay, Ph.D., F.R.S. Received April 26, 1894.

Since the publication of a research on the molecular complexity of liquids by Ramsay and Shields ('Trans. Chem. Soc.,' vol. 63, p. 1191) two questions have arisen:—First: What other evidence is there as to the existence of complex molecules in certain liquids?

Second: How can the amount of dissociation of associating liquids be inferred from measurements of their surface-energy?

The first of these questions has been treated of by Professor Philippe Guye, in the 'Archives des Sciences Physiques et Naturelles de Genève,' 31; and as that periodical is not easily accessible to English readers, a short account of his article is given here.

I. *Evidence in favour of the Molecular Complexity of Certain Liquids.*

a. Guye has shown ('Annales,' vol. 31, [6], p. 206) that the quotient obtained by dividing the absolute critical temperature of a liquid by the critical pressure measured in atmospheres is equal to the molecular refraction of the liquid multiplied by a factor which is approximately equal to 1·8. His reason for this statement is as follows:—Equations such as that of van der Waals, which express approximately the constants for gases and liquids in terms of temperature, pressure, and volume, assume as one of their data the "co-volume" (b) of the substance; *i.e.*, a number proportional to the actual volume of the molecules, supposing them to be spherical. The dielectric constant of a body k, according to Clausius, depends on the ratio u of the real volume to the apparent volume occupied by the molecules, in such a manner that

$$u = (k-1)/(k+2).$$

Maxwell has shown that according to the electro-magnetic theory of light, the dielectric constant k should be equal to the square of its index of refraction for a ray of light of infinite wave-length; hence

$$u = \frac{(n^2-1)}{(n^2+2)}.$$

The name molecular refraction is given to this quantity referred to the volume of 1 gram, and multiplied by the molecular weight, or

$$\frac{u}{d}\mathrm{M} = \frac{(n^2-1)}{(n^2+2)} \cdot \frac{\mathrm{M}}{d} = \mathrm{MR},$$

where MR signifies molecular refraction.

Now van der Waals has shown that the relations between critical temperature, pressure, and volume are given by the equation

$$\tfrac{3}{8}(1+\alpha\theta) = \frac{\pi\phi}{(1+3\pi\phi^2)(1-\phi/3)}.$$

The denominator on the right-hand side of the equation is nearly equal to unity. Assuming this to be the case, and introducing the value of $\alpha = \dfrac{1}{273}$,

$$k = \frac{273+\theta}{\pi} = \tfrac{8}{3}\,.\,273\phi,$$

where k is the "critical coefficient," or the critical temperature divided by the critical pressure.

Now the value of ϕ is related to b by the equation

$$\phi = 3b,$$

and b being proportional to the molecular refraction, the relation

$$k = \frac{273+\theta}{\pi} = \frac{1}{f}\cdot\frac{n^2-1}{n^2+2}\cdot\frac{M}{d} = \frac{1}{f}MR$$

should hold. That is, k, the quotient obtained on dividing the absolute critical temperature by the critical pressure, should, when multiplied by a constant, be equal to the molecular refraction.

While the majority of substances examined by Guye appear to consist of simple molecular groups at their critical points, water, methyl alcohol, and acetic acid yield numbers which point to association, inasmuch as the constant f, instead of having its usual value 1·8, has decreased to about 1·1.

b. The densities of most liquids at their critical points may be found by multiplying their theoretical densities by a number approximately equal to 3·85 (Young and Thomas, 'Trans. Chem. Soc.,' 1893, p. 1251; also 'Phil. Mag.,' 1892, p. 507). But for a few substances the following values were found:—

Methyl alcohol..........	4·52
Ethyl alcohol...........	4·02
Propyl alcohol..........	4·02
Acetic acid.............	5·00

The factor should be greater, if association occurs, because the theoretical density calculated by Boyle's and Gay-Lussac's laws would then be greater than if it were supposed that the molecules of methyl alcohol, for example, were represented by the simple formula CH_4O. Here, again, the evidence points to complex molecules at the critical temperature.

c. Cailletet and Matthias have suggested a simple plan for finding the true volume of a substance at its critical point. It consists in mapping the densities in the state of liquid and of gas against temperature (as seen, for example, in the diagram given by Ramsay and Young in their memoir on alcohol in the 'Philosophical Transactions,' 1886, Part I, plate 7), and bisecting the lines of equal pressure, which cross the diagram horizontally. Such lines are lines of equal pressure at constant temperature. On joining the points where the lines are bisected, a straight line is obtained in the case of most liquids, which, when continued vertically, cuts the curve at the critical density. But to this rule Young and Thomas find that water, and methyl, ethyl, and propyl alcohols are exceptions, for they give curved lines. These substances are not associated in the state of gas, although in the liquid state they display association. Acetic acid, however, which displays association both in the state of liquid and of gas, gives a line which is, if not quite, at least very nearly straight.

It may therefore be concluded that while a curved line implies association in the state of liquid, a straight line implies either no association or association in both conditions.

d. The heat required to vaporise a dissociating liquid is employed in two ways when the gas, as is always the case, has a simpler molecular formula than the liquid. A portion of the heat is employed in vaporisation alone; while a portion is absorbed in effecting the decomposition of complex molecular groups. The heat of vaporisation alone diminishes as temperature rises, till at the critical point it is zero; but the heat required to dissociate molecular groups may increase, if that term is of importance, and may cause the total heat to increase. The researches of Ramsay and Young on ethyl alcohol and on acetic acid have shown that there exist maxima in the heats of vaporisation of these substances. Thus at 0°, the heat of vaporisation of ethyl alcohol is 220·9 cal.; at 10°, 221·2; at 20°, 220·6; and at 30°, 220·1. The numbers then decrease as usual. With acetic acid at 80°, the value is 91·6 cal.; at 100°, 92·3; at 110°, 92·8; at 120°, 92·7; at 130°, 92·4, and so on. It may be stated, then, that when the numbers representing heats of vaporisation of a compound increase to a maximum, and then diminish, the compound contains complex molecules in the liquid state. It does not follow that all substances which possess complex liquid molecules must exhibit such a maximum, for this peculiarity evidently depends on the relative importance of the heats of dissociation and of vaporisation.

e. The curves representing the vapour pressures of non-dissociating liquids do not cut one another at any point in their course. Liquids which associate give vapour-pressure curves which cut some of those

of non-dissociating liquids and frequently cut those of dissociating liquids. The fact, then, that the vapour-pressure curve of a liquid cuts those of undeniably simple substances, such as benzene, carbon tetrachloride, &c., may be taken as a proof that that liquid contains complex molecules.

f. This relation may also be expressed by the factor in van der Waals' equation for calculating vapour-pressures, viz.:—

$$\log pc - \log p = f\frac{Tc - T}{T},$$

where pc is the critical pressure; Tc the critical temperature; and p some other pressure at temperature T. The constant f has a value close to 3 for all non-associating compounds. Thus from Young's results the following values of f are calculated:—

	f.		f.
Benzene	2·89	Propyl formate	3·04
Chlorobenzene	2·95	Methyl acetate	3·07
Fluorobenzene	2·99	Ethyl acetate	3·26
Carbon tetrachloride	2·81	Propyl acetate	3·22
Tin tetrachloride	3·01	Methyl propionate	3·13
Ethyl oxide	3·00	Ethyl propionate	3·22
Methyl formate	3·00	Methyl butyrate	3·25
Ethyl formate	2·97	Methyl isobutyrate	3·15

The mean value is 3·06.

But for liquids with complex molecular groupings the values are considerably higher, and, moreover, are not constant.

	f.
Methyl alcohol	3·56 to 3·77
Ethyl alcohol	3·58 „ 4·02
Propyl alcohol	3·49 „ 3·77
Acetic acid	3·36 „ 3·49
Water	3·20 „ 3·24

Other relations besides those mentioned by Guye, of whose memoir the preceding pages give an abstract, also point towards the molecular complexity of the alcohols and acids. Among them may be mentioned the ratios of the volumes of saturated vapour at some chosen pressure to that at the critical pressure, as shown in p. 1257 of Young's memoir (*loc. cit.*); the greater values of the expression $(dp/dt)T$ for the alcohols and for water compared with those of other substances (see Ramsay and Young, 'Proc. Phys. Soc.,' VII, p. 303); this really means the greater heat of vaporisation for unit increase of volume, for $(dp/dt)T$ is equivalent to $L/(S_1 - S_2)$. This

fact indeed is pointed out in the first part of the series of papers (*ibid.*, 291). Again the ratios of total to external work produced on evaporation (*ibid.*, 293) show that the total work is a higher multiple of the external work or work employed in expansion against pressure, in the case of the alcohols, acetic acid, and water, than in the case of other compounds.

Enough has been said to show that a great mass of evidence exists in favour of molecular complexes in certain liquids. It remains now to consider the methods by which the degree of complexity can be ascertained.

II. *Methods of Deducing the Molecular Complexity of Liquids from Measurements of their Molecular Surface-energy.*

It was shown by Ramsay and Shields ('Phil. Trans.,' 1893, A, 662) that the relation of molecular surface-energy of many liquids to temperature may be expressed by the equation

$$(1) \qquad \gamma\,(Mv)^{\frac{2}{3}} = k\,(\tau - d),$$

where γ is surface-tension, measured in dynes, $(Mv)^{\frac{2}{3}}$ the molecular surface measured in square centimetres, k is an approximate constant for most liquids varying little from 2·12, and τ is the temperature numbered downwards from the critical point; d is a nearly constant number of degrees, usually 5°, which must be subtracted from τ.

For liquids which associate, such as the alcohols and fatty acids, the value of k is not constant, but increases with rise of temperature.

The problem is, knowing the average value of k for non-associating liquids, to deduce the average molecular weights of associating liquids at any given temperature.

Differentiating equation (1) we obtain

$$\frac{d}{dt}\cdot\gamma\,(Mv)^{\frac{2}{3}} = k,$$

for non-associating liquids; or, if we insert a term x, to represent a factor with which the gaseous or normal molecular weight of a liquid should be multiplied in order that the normal value of k should result from the equation, we obtain

$$(2) \qquad \frac{d}{dt}\cdot\gamma\,(xMv)^{\frac{2}{3}} = k,$$

In our first attempts to deduce the true average value of M for associating liquids, equation (2) was expanded, thus:—

$$(3) \qquad x^{\frac{2}{3}}\,.\,\frac{d}{dt}\gamma\,(Mv)^{\frac{2}{3}} + \gamma\,(Mv)^{\frac{2}{3}}\,.\,\frac{dx^{\frac{2}{3}}}{dt} = k\,;$$

and it was assumed, as a first approximation, that the second term, the variation of $x^{\frac{2}{3}}$ with temperature, was negligible. In such a case

$$(4) \qquad x = \left\{ k \frac{dt}{d\gamma\,(\mathrm{M}v)^{\frac{2}{3}}} \right\}^{\frac{3}{2}};$$

and it was on this assumption that the results given in the papers referred to for the alcohols, the acids, and water were calculated.

The numbers obtained were, however, as will be shown, much in excess of the truth.

An attempt was made to approximate to the true value of x, by calculating it by means of equation (4) approximately, and using the results obtained to correct equation (3), by inserting the neglected second term. This was found to be impossible, and to lead to absurd results; hence it was inferred that the variation of x with temperature was such as to make it imperative that attention should be paid to the second term of equation (3). At the same time it was noticed in mapping x that its alteration with temperature was approximately linear; and this fact greatly simplified the problem.

Mr. J. Rose-Innes, who has taken much interest in this work, and has on several occasions given valuable assistance, was kind enough to endeavour to find an expression which would satisfy these conditions.

A formula of the form

$$(5) \qquad \gamma \mathrm{M}v^{\frac{2}{3}} = \frac{k\,(\tau - d)}{1 + \mu\tau}$$

agrees admirably with the experimental values of molecular surface-energy for methyl and ethyl alcohols, water, and acetic acid between low temperatures and some 30° below their critical points. Even at —89·8°, it will be noticed, the agreement for methyl and ethyl alcohols is reasonably good.

The constants for these substances are :—

	k.	*d*.	*μ*.	Critical temperature.
Methyl alcohol......	1·489	—4·22	0·00104	240·0°
Ethyl alcohol.......	2·170	4·8	0·00193	243·1
Water.............	2·631	19·5	0·00218	358·1
Acetic acid.........	1·910	11·9	0·00163	321·5

A comparison between the calculated and found values of $\gamma(\mathrm{M}v)^{\frac{2}{3}}$ is given in the following table :—

Methyl alcohol.				Ethyl alcohol.				Water.				Acetic acid.			
T.	r.	$\gamma(Mv)^{\frac{2}{3}}$.		T.	r.	$\gamma(Mv)^{\frac{2}{3}}$.		T.	r.	$\gamma(Mv)^{\frac{2}{3}}$.		T.	r.	$\gamma(Mv)^{\frac{2}{3}}$.	
		Found.	Calctd.			Found.	Calctd.			Found.	Calctd.			Found.	Calctd.
°	°			°	°			°	°			°	°		
−89·8	329·8	361·8	369·8	−89·8	332·9	436·1	433·3	0	368·1	502·9	503·5	20	301·5	371. 2	371·1
+20	220	271·4	271·4	+20	223·1	331·0	331·0	20	338·1	485·3	484·9	40	281·5	..	353·2
70	170	216·2	220·4	40	203·1	307·3	309·0	40	318·1	466·3	466·3	60	261·5	..	334·4
90	150	196·3	198·6	60	183·1	284·8	285·8	60	298·1	446·2	446·4	80	241·5	..	314·8
110	130	176·7	176·0	80	163·1	261·2	261·1	80	278·1	425·3	425·5	100	221·5	..	294·3
130	110	154·8	152·5	100	143·1	235·0	235·1	100	258·1	403·5	403·5	120	201·5	..	272·7
150	90	131·3	128·3	120	123·1	208·0	207·3	120	238·1	380·7	380·3	140	181·5	250·2	250·0
170	70	104·8	103·0	140	103·1	178·8	177·8	140	218·1	357·0	355·6	160	161·5	226·3	226·1
180	60	91·0	90·0	160	83·1	147·2	146·4					180	141·5	200·2	201·2
190	50	76·1	76·8	180	63·1	112·6	112·7					200	121·5	174·9	174·7
200	40	60·6	63·2	200	43·1	75·7	76·7					220	101·5	146·9	146·9
210	30	45·4	49·4	210	33·1	57·1	57·7					240	81·5	117·5	117·3
220	20	29·2	35·3	220	23·1	39·2	38·0					260	61·5	86·0	86·1
				230	13·1	19·8	17·6					280	41·5	54·8	52·9

It may be remarked that at temperatures within 20 or 30 degrees of the critical point the former no longer accurately expresses the results. This is not peculiar to associating compounds, as has already been shown in the 'Phil. Trans.,' *loc. cit.*, p. 657. Should it be desired to secure more accurate correspondence between the found data near the critical temperatures and those calculated, the last term may be modified. The equation then becomes

$$\gamma(\mathrm{M}v)^{\frac{2}{3}} = \frac{k\tau - kd\,(1-10^{-\lambda\tau})}{1+\mu\tau}.$$

For ethyl alcohol, the value of λ is 0·044, and, on introducing this correction, the calculated values near the critical point, above 180°, are as follow :—

T.	τ.	$\gamma \mathrm{M}v^{\frac{2}{3}}$.		T.	τ.	$\gamma \mathrm{M}v^{\frac{2}{3}}$.	
		Found.	Calctd.			Found.	Calctd.
°	°						
240	3·1	3·7	3·9	200	43·1	75·7	76·9
236	7·1	9·9	9·9	190	53·1	94·9	95·1
234	9·1	13·3	13·2	180	63·1	112·6	112·8
230	13·1	19·8	20·3	178	73·1	130·1	129·9
220	23·1	39·2	39·0	160	83·1	147·2	146·4
210	33·1	57·1	58·1	150	93·1	163·0	162·5

Similar corrections could be introduced for methyl alcohol, acetic acid, and water, which would have the effect of reproducing the experimental numbers at low values of τ.

The following considerations show how it is possible to calculate the degree of association of such compounds at any desired temperature. Neglecting for the present the "λ" term, which is introduced to secure concordance at temperatures near the critical point, let us consider equation (1), where k has the value 2·121 for unassociating liquids, viz.,

$$\gamma(\mathrm{M}v)^{\frac{2}{3}} = 2{\cdot}121\,(\tau - d).$$

Supposing that the liquid is composed partly of complex molecules, and that x is a measure of the complexity, we should have

$$\gamma\,(x\mathrm{M}v)^{\frac{2}{3}} = 2{\cdot}121\,(\tau - d),$$

or

$$\gamma\,(\mathrm{M}v^{\frac{2}{3}}) = 2{\cdot}121 \times \frac{1}{x^{\frac{2}{3}}} \times (\tau - d).$$

Comparing this with equation (5), which reproduces the results for associating liquids with fair accuracy,

$$\gamma\,(\mathrm{M}v)^{\frac{2}{3}} = k\,(\tau - d)/(1+\mu\tau),$$

it is evident that x corresponds to the expression

$$\left\{\frac{2\cdot121}{k}\,(1+\mu\tau)\right\}^{\frac{3}{2}}.$$

It is, of course, easy to include the "λ" term, when x follows, as before.

There can, I think, be no doubt that this method gives a correct value to the factor of association, within certain limits. These limits are conditioned by the fact that the number chosen for k, viz., 2·121, is not absolutely constant, but varies with the nature of the compound. The extreme variation found for the fourteen substances which have been most carefully investigated is between 2·020 for ethyl formate, and 2·248 for methyl isobutyrate. On the assumption that this is the extreme divergence, there may be an error of 5 per cent. in a negative or positive direction caused by assuming the mean value 2·121.

But there is another assumption involved in such calculations. It is that a mixture of two liquids possesses such a molecular surface-energy that the mean molecular weight of the mixture, calculated from the proportion in which they are present in the mixture, shall be deducible from the molecular surface-energy. It is conceivable that the surface of such a mixture should not exhibit the same distribution of molecules as the interior, and evidence is required to show that the assumption that it does is correct. This evidence is given in another communication, and it appears therefore that the assumption is justified.

With these premises, therefore, I proceed to give the molecular association of methyl and ethyl alcohols, water, and acetic acid.

Methyl alcohol.			Ethyl alcohol.			Water.			Acetic acid.		
T.	τ.	x.	T.	τ.	x.	T.	τ.	x.	T.	τ.	x.
− 89·8°	329·8°	2·65	− 89·8°	332·9°	2·08	0°	358·1°	1·707	20°	301·5°	2·13
+ 20	220	2·32	+ 20	223·1	1·65	20	338·1	1·644	40	281·5	2·06
70	170	2·17	40	203·1	1·59	40	318·1	1·582	60	261·5	1·99
90	150	2·11	60	183·1	1·52	60	298·1	1·523	80	241·5	1·92
110	130	2·06	80	163·1	1·46	80	278·1	1·463	100	221·5	1·86
130	110	2·00	100	143·1	1·39	100	258·1	1·405	120	201·5	1·79
150	90	1·94	120	123·1	1·33	120	238·1	1·346	140	181·5	1·72
170	70	1·89	140	103·1	1·27	140	218·1	1·289	160	161·5	1·66
180	60	1·86	160	83·1	1·21	..	..	..	180	141·5	1·59
190	50	1·83	180	63·1	1·15	..	..	..	200	121·5	1·53
200	40	1·81	200	43·1	1·09	..	..	..	220	101·5	1·47
210	30	1·78	210	33·1	1·06	..	..	..	240	81·5	1·41
220	20	1·75	220	23·1	1·03	..	..	..	260	61·5	1·35
			230	13·1	1·00	..	..	..	280	41·5	1·30

Addendum.

Since the foregoing pages were written, Professor van der Waals has published a long memoir on the "Thermodynamic Theory of Capillarity on the Assumption of Continuous Change of Density" ('Zeitschrift für physikalische Chemie,' vol. 13, pp. 657—725). On the main part of his work I have no criticism to offer; but on p. 714, he states some objections to the method previously employed by Ramsay and Shields in calculating the factor of association x. These remarks are fully justified, as will have been seen from the preceding pages of this paper; but in the formula which he suggests to replace it, he makes an assumption which, at first sight, is no less untenable than our assumption that the factor of association, x, does not vary with temperature. In placing the factor of association as equal to unity at temperatures near the critical temperature, he obtains the formula

$$x^{\frac{1}{2}} = \frac{k(\tau' - \tau) + \gamma(\mathrm{M}v)^{\frac{2}{3}}}{\gamma(\mathrm{M}v)^{\frac{2}{3}}},$$

and, inasmuch as this assumption is apparently very nearly true for methyl and ethyl alcohols and for acetic acid, the numbers he gives are nearly identical with those in the last table of this paper. But they differ in the case of water, and x, according to him, is equal to 1·9, instead of to 1·707.

The formula given by him on p. 716 to calculate γ yields remarkably good results. In fact, if γ be calculated for ether at $-89{\cdot}8^\circ$, a result is obtained identical with that found. This result was unforeseen by van der Waals, for the value for γ, 30·65, was not given by us at that low temperature in our previous paper.

Professor van der Waals, however, makes two criticisms which appear to me to be hardly justified. The first refers to a correction applied by us in order to allow for the capillarity in the wider, yet still narrow, tube in which the capillary tube stood. He thinks that this correction would be affected by the curvature of the meniscus not being the same at high as at low temperatures. The remark is certainly true; but as the alteration in height due to altered curvature of meniscus would be well within the range of experimental error, it is negligible. The second criticism deals with the capillarity near the critical point, and van der Waals states that the simple formula, applicable to narrow tubes, no longer holds when the capillary rise is only a few times greater than the radius of the tube. This objection would be justified were it not that the capillary heights are nearly a linear function of the temperature; and with non-dissociating liquids, which he is here considering,

it is quite unnecessary to take measurements at temperatures very close to the critical temperature, because, if that temperature is known, there can be only one curve joining the points experimentally found at somewhat lower temperatures and the critical temperature. The form of the curve is such that no doubt can exist as to its course. Indeed, with chlorobenzene, measurements were not carried out at all in the immediate neighbourhood of the critical point, but only at much lower temperatures, and yet there could be no question as to the course of the curve, when it was mapped. But these are minor points; and it is very gratifying to find that the material provided by Dr. Shields and myself affords such a remarkable confirmation of the justice of Professor van der Waals' views.—18*th May*, 1894.

III. "The Molecular Surface-Energy of Mixtures of non-associating Liquids." By Professor WILLIAM RAMSAY, Ph.D., F.R.S., and Miss EMILY ASTON, B.Sc. Received April 26, 1894.

It has been shown in the previous paper that it is possible to calculate the degree of association of an associating liquid such as alcohol, on the assumption that molecules of less complexity remain uniformly distributed along with molecules of greater complexity throughout the liquid, and that no one kind of molecule tends to congregate on the surface to the exclusion of the other. It is necessary, however, to justify this assumption; and for this reason experiments have been made on mixtures of liquids the molecules of which do not unite to form complex groups; such are most of the liquids investigated by Ramsay and Shields ('Trans. Chem. Soc.,' vol. 63, p. 1099, *et seq.*).

The experiments of which an account is here given, show that while the height to which a mixture of two liquids ascends in a capillary tube is not the mean of the heights to which each singly would ascend at the same temperature, while the surface-tensions and the surface-energies are not necessarily the mean of those possessed by the liquids unmixed with each other, regard being paid to their relative proportion in the mixture, yet the coefficient of decrease of molecular surface-energy, and consequently the calculated molecular weights, are true means of those of the two liquids.

The substances used in these experiments were chosen in pairs, and as it was necessary in closing the tubes to evaporate some of the contained liquid in order to ensure the expulsion of air, mixtures of such liquids were taken as possess approximately equal boiling points, so

that each should evaporate to nearly the same extent. For this reason the following liquids were chosen:—

	{ Toluene............	110°·6 at 761·2 mm. pressure.
	{ Piperidine	105—106°·2 at 769 mm. pressure.
*	{ Benzene	Constant, about 80°.
	{ Carbon tetrachloride	Constant „ 77°.
	{ Chlorobenzene......	Constant „ 132°.
	{ Ethylene dibromide .	Within 0·5°, about 131°.
	{ Carbon disulphide...	Constant, about 46·2°.
	{ Chloroform	Constant, about 62°.

The amount of toluene distilled was 750 c.c. The thermometer did not vary during the distillation by the fiftieth part of a degree. The amount of piperidine was much less, about 75 c.c. The alteration of boiling point appears to be due, in part at least, to its eager absorption of carbon dioxide. The benzene was part of a large stock which had been repeatedly frozen and thawed. It was free from thiophene, and had an absolutely constant boiling point. The carbon tetrachloride boiled constantly while 400 c.c. passed over. The chlorobenzene was part of a stock used for securing constant temperatures, and had been repeatedly fractionated; it boiled with absolute constancy while 750 c.c. passed over. The ethylene dibromide was not quite so pure; the rise of 0·5°, however, was spread over 200 c.c.; while the purity of the carbon disulphide and the chloroform was guaranteed by the constancy of boiling point while large quantities distilled.

The molecular surface-energies of the pure substances were first determined. They are given in the tables which follow:—

t = temperature.
h = rises in centimetres in capillary tube.
ρ = density of liquid.
γ = surface-tension calculated by the equation $\gamma = \frac{1}{2}rgh\rho$.
r = radius of tube.
$\gamma(\text{M}v)^{\frac{2}{3}}$ = molecular surface-energy, where
M = molecular weight, and
v = volume of one gram.

* The constancy of the boiling-point of a liquid is the guarantee of its purity, provided a considerable quantity boils at a constant temperature. The determination of the actual temperature involves the accuracy of the thermometer.

$r = 0·01843$ cm.

Toluene.						Piperidine.					
t.	h.	ρ.	γ.	$\gamma(Mv)^{\frac{2}{3}}$.	k.	t.	h.	ρ.	γ.	$\gamma(Mv)^{\frac{2}{3}}$.	k.
15°·2	3·590	0·8682	28·18	631·0	2·137	15°·2	3·785	0·8670	29·67	635·6	2·068
46·6	3·247	0·8380	24·60	563·9	2·299	46·6	3·466	0·8363	26·21	570·7	2·168
78·4	2·867	0·8080	20·93	490·8	2·007	78·4	3·087	0·8048	22·46	501·8	2·055
132·5	2·280	0·7535	15·53	382·2		132·5	2·457	0·7256	16·72	390·6	

$r = 0·01843$ cm.

Benzene.						Carbon tetrachloride.					
t.	h.	ρ.	γ.	$\gamma(Mv)^{\frac{2}{3}}$.	k.	t.	h.	ρ.	γ.	$\gamma(Mv)^{\frac{2}{3}}$	k.
11°·2	3·642	0·8871	29·21	575·6	2·104	11°·8	1·861	1·6063	27·03	565·7	2·125
46·0	3·213	0·8500	24·71	502·3	2·162	46·0	1·643	1·5420	22·90	492·6	2·072
78·0	2·810	0·8147	20·70	433·1		78·0	1·442	1·4771	19·26	426·3	

$r = 0{\cdot}01046$ cm.

Chlorobenzene.						Ethylene dibromide.					
t.	h.	ρ.	γ.	$\gamma(Mv)^{\frac{2}{3}}$.	k.	t.	h.	ρ.	γ.	$\gamma(Mv)^{\frac{2}{3}}$.	k.
9°·5	5·875	1·1182	33·71	729·1		12°·2	3·467	2·1873	38·91	757·7	
45·6	5·290	1·0795	29·30	648·8	2·225	44·9	3·180	2·1189	34·57	687·7	2·140
77·8	4·790	1·0444	25·66	581·0	2·104	77·2	2·887	2·0502	30·37	617·6	2·170
131·3	3·950	0·9836	19·93	469·8	2·079	131·3	2·395	1·9315	23·74	502·2	2·133

$r = 0{\cdot}01046$ cm. $r = 0{\cdot}01708$.

Chloroform.						Carbon disulphide.					
t.	h.	ρ.	γ.	$\gamma(Mv)^{\frac{2}{3}}$.	k.	t.	h.	ρ.	γ.	$\gamma(Mv)^{\frac{2}{3}}$.	k.
10°·2	3·570	1·5077	27·62	509·7		9°·7	3·057	1·2773	32·73	499·2	
45·5	3·120	1·4385	23·03	438·5	2·016	46·0	2·700	1·2224	27·68	434·8	1·802
77·6	2·700	1·3773	19·98	374·0	2·010	61·0	2·525	1·1980	25·35	403·6	2·078

These results call for no special remark, except that, contrary to the experiments of Ramsay and Shields, carbon disulphide appears to associate somewhat at low temperatures. Further experiments will be made on this matter at still lower temperatures. The result given here may be taken as reliable, for it was carefully repeated several times, special precautions being taken to ensure the absolute purity of the bisulphide, and using a different capillary tube.

The densities were taken from the following sources:—

Toluene, Nasini and Pagliani, 'Jahresb.,' 1862, p. 63.

Piperidine, Beilstein, 'Organische Chemie,' vol. 3, p. 616.

Benzene, Kopp, 'Annalen,' vol. 64, p. 215.

Carbon tetrachloride, Thorpe, 'Trans. Chem. Soc.,' vol. 37, p. 200.

Chlorobenzene, determined by ourselves at the temperatures chosen.

Ethylene dibromide, Thorpe, *ibid.*, p. 197.

Chloroform, *ibid.*, p. 197.

Carbon disulphide, *ibid.*, p. 364.

Determination of the Capillary Rise of Mixtures.

I. *Toluene and Piperidine.*

(*a.*) $5C_6H_5.CH_3$ to $1C_5H_{10}$:NH.

In filling the tube with this mixture 0·113 gram was lost out of a total of 4 grams, or a little over 2 per cent. It may be assumed that, owing to these liquids having so nearly the same boiling point, no material alteration of their ratio is due to this cause. The density of the mixture was assumed to be the mean of those of the constituents, taken in the proportion in which they were present. As will be afterwards shown, no appreciable error is involved in this assumption. The values of *h* are the mean of four observations in each case.

In the columns with the heading "calculated" the mean height, surface-tension, and surface-energy have been inserted, together with the mean values of *k*.

$5C_6H_5 \cdot CH_3$ to $1C_5H_{10}$:NH.

Found.						Calculated.			
t.	*h.*	ρ(calctd.).	γ.	$\gamma(Mv)^{\frac{2}{3}}$.	*k.*	*h.*	γ.	$\gamma(Mv)^{\frac{2}{3}}$.	*k.*
14°·5	3·647	0·8684	28·63	635·5	2·191	3·622	28·63	631·8	2·079
46·6	3·285	0·8377	24·88	565·6	2·032	3·283	24·53	565·0	2·277
78·4	2·945	0·8077	21·54	501·0	2·123	2·904	21·19	492·6	2·013
132·5	2·323	0·7535	15·82	386·1		2·309	15·73	383·7	

The mean value of K, calculated over the whole range of temperature, is 2·115; the mean value found is 2·123; hence it may be concluded that these liquids are without influence on one another. It will also be noticed that the found and calculated values of γ and of $\gamma(Mv)^{\frac{2}{3}}$ are in very close correspondence.

To check these results the proportions were reversed, and a mixture of

(*b*) $5C_5H_{10}{:}NH$ to $1C_6H_5CH_3$ was investigated.

$5C_5H_{10}{:}NH$ to $1C_6H_5CH_3$.

Found.						Calculated.			
t.	*h.*	ρ(calctd.).	γ.	$\gamma(Mv)^{\frac{2}{3}}$.	*k.*	*h.*	γ.	$\gamma(Mv)^{\frac{2}{3}}$.	*k.*
14°·9	3·757	0·8674	29·46	632·0	2·094	3·752	29·42	634·9	2·060
46·6	3·404	0·8366	25·74	565·6	2·069	3·429	25·94	569·6	2·190
78·4	3·046	0·8052	22·17	499·8	2·056	3·050	22·21	499·9	2·047
132·5	2·422	0·7527	16·48	388·6		2·427	16·52	389·2	

The mean value of *k*, calculated over the whole range of temperature, is 2·087; the mean value found is 2·067. Here again the found values of surface-tension and of surface-energy agree very closely with those calculated.

It is possible, without assuming a molecular weight, to calculate the mean molecular weights of such mixtures by means of the equation

$$M = \left\{ \frac{k(t'-t)}{\gamma v^{\frac{2}{3}} - \gamma' v'^{\frac{2}{3}}} \right\}^{\frac{3}{2}}.$$

In doing this, the mean value of *k* for each mixture has been taken, and the value of M has been calculated between extreme limits of temperature. For the first mixture the mean molecular weight found is 90·61; that calculated for a mixture of five molecules of toluene to one of piperidine is 90·83; for the second mixture the mean molecular weight found is 86·12, and the calculated value for a mixture of five molecules of piperidine to one of toluene is 86·17. This, of course, constitutes only an arithmetical check on the other results, but if a mean value for *k* had been chosen, *e.g.*, 2·121, the results would have been practically the same.

It is obvious that in this case the liquids are without influence on each other.

II. *Benzene and Carbon Tetrachloride.*

(*a*.) $1C_6H_6$ to $1CCl_4$.

Similar experiments were made with the above mixture; the densities of the mixture were determined experimentally, and a comparison is given at each temperature of the numbers found, with the mean density of the mixture, calculated from the found densities of the components. The loss of weight on sealing showed that 4·5 per cent. of the mixture had evaporated; but here, too, the boiling points of benzene and of carbon tetrachloride are so near that it is probable that no important change in composition occurred.

$1C_6H_6$ to $1CCl_4$.

t.	h. Found.	h. Calctd.	ρ. Found.	ρ. Calctd.	γ. Found.	γ. Calctd.	$\gamma(Mv)^{\frac{2}{3}}$. Found.	$\gamma(Mv)^{\frac{2}{3}}$. Calctd.	k. Found.	k. Calctd.
16°·0	2·421	2·709	1·2597	1·2558	27·70	27·58	561·9	562·4	2·331	2·148
46·2	2·150	2·428	1·2095	1·2098	23·50	23·80	492·0	497·5	2·110	2·118
78·2	1·880	2·126	1·1596	1·1590	19·71	19·98	424·5	429·7		

The calculated values of h are the means of the heights of benzene and carbon tetrachloride at the respective temperatures. It is to be noticed that the observed heights are widely different. But, owing to the different densities of the two liquids, the calculated values of γ, the surface-tensions, are nearly the means of those of each taken singly; and the agreement of the found and calculated molecular surface-energy, $\gamma(Mv)^{\frac{2}{3}}$, is also a close one. The value of k exaggerates the error of experiment, yet, on the whole, the agreement is satisfactory. It would also appear that the operation of mixing does not affect the density of either liquid appreciably.

(*b*) and (*c*). The tables which follow show the effect of varying the relative proportions :—

(*b*.) $10C_6H_6$ to $17CCl_4$.

t.	h. Found.	h. Calctd.	ρ. Found.	ρ. Calctd.	γ. Found.	γ. Calctd.	$\gamma(Mv)^{\frac{2}{3}}$. Found.	$\gamma(Mv)^{\frac{2}{3}}$. Calctd.	k. Found.	k. Calctd.
13°·2	2·265	2·508	1·3509	1·3505	27·66	27·64	568·1	566·9	2·127	2·119
46·6	2·010	2·224	1·2942	1·2963	23·51	23·57	497·0	496·1	2·290	·119
78·4	1·740	1·948	1·2411	1·2422	19·52	19·79	424·2	428·7		

(*c.*) $2C_6H_6$ to $1CCl_4$.

	h.		*ρ.*		*γ.*		*γ(Mv)* .		*k.*	
t.	Found.	Calctd.	Found.	Calctd.	Found.	Calctd.	Found.	Calctd.	Found.	Calctd.
10°·8	2·769	3·052	1·1384	1·1395	28·55	28·37	576·2	574·1	2·198	2·083
46·2	2·440	2·689	1·0877	1·0899	23·99	24·11	499·5	499·1	2·231	2·133
78·2	2·121	2·354	1·0431	1·0445	20·00	20·22	428·1	430·8		

The same remarks apply to these results.

The mean values of *k*, calculated over the whole range of temperature are given in the next table, together with the values of M, also deduced between the extremes of temperatures by the equation already given.

	k.		M.	
	Found.	Calculated.	Found.	Calculated.
a.	2·125	2·131	113·1	112·9
b.	2·207	2·124	125·8	125·72
c.	2·077	2·106	103·5	103·3

Here, again, within limits of experimental error, it is seen that the values of γ of $\gamma(Mv)^{\frac{2}{3}}$, and of *k* are uninfluenced by the operation of mixing, and that the mean molecular weight of the mixture is calculable from the data found.

III. *Mixtures of Chlorobenzene and Ethylene Dibromide.*

A mixture of equal molecular proportions of these liquids gave the following results:—

t.	h.		ρ.		γ.		$\gamma(Mv)^{\frac{2}{3}}$.		k.	
	Found.	Calctd.	Found.	Calctd.	Found.	Calctd.	Found.	Calctd.	Found.	Calctd.
10°·2	4·292	4·666	1·6064	1·6107	35·38	36·40	729·0	748·5	2·147	2·190
45·6	3·890	4·232	1·5519	1·5564	30·97	31·89	653·0	671·0	2·152	2·142
77·8	3·515	3·836	1·5014	1·5056	27·08	27·98	583·7	602·0	1·989	2·113
131·3	2·925	3·173	1·4154	1·4184	21·23	21·83	477·3	486·9		

The radius of the tube was 0·01046 cm.

The calculated height given is the mean of the heights of chlorobenzene and ethylene dibromide, corrected for temperature-difference, on the assumption (which is practically without error for such small differences) that the variation of height with temperature is a linear one. The calculated density is a similar mean. But these heights and densities are not made use of in calculating the values of the "calculated" γ. It, too, is the mean of the found values (see p. 184), and similarly the values of $\gamma(Mv)^{\frac{2}{3}}$ are calculated from the calculated values of γ, and from the calculated densities.

The molecular weight, computed by means of the equation given on p. 187, for the whole range of temperature employed, is 148·6 instead of the theoretical mean 150·25.

IV. *Mixture of Chloroform and Carbon Disulphide.*

Here also equal molecular proportions were used.

$$r = 0·01046 \text{ cm.}$$

t.	h.		ρ.		γ.		$\gamma(Mv)^{\frac{2}{3}}$.		k.	
	Found.	Calctd.	Found.	Calctd.	Found.	Calctd.	Found.	Calctd.	Found.	Calctd.
9°·0	4·050	4·723	1·4026	1·4132	29·16	30·30	493·7	510·6	1·847	1·897
44·9	3·560	3·788	1·3406	1·3496	24·49	25·47	427·4	442·5	2·168	2·062
61·0	3·300	3·520	1·3128	1·3213	22·23	23·23	392·5	409·3		

As the value of k for this mixture points to association at low temperatures, the mean molecular weight has not been calculated.

We see, from these experiments, that a mixture of two liquids may either behave as a mean, or the liquids may influence each other. With the first two pairs, toluene and piperidine, and benzene and carbon tetrachloride, the liquids belong to very different chemical types. With the second pair, the heights and densities differ very greatly from each other, and although the values of γ and of $\gamma(Mv)^{\frac{2}{3}}$ approach more nearly, there is still a marked difference. Yet the values of ρ, the density, γ, the surface-tension, and $\gamma(Mv)^{\frac{2}{3}}$, the molecular surface-energy of the mixtures, are identical with those calculated, within limits of experimental error. Perhaps this statement should, in strictness, not apply to the densities, yet there is no great divergence from the mean.

It is therefore legitimate to state that in certain cases the molecular surface energy of a mixture is the mean of those of its constituents determined at the same temperature.

The third pair of liquids, chlorobenzene and ethylene dibromide, give results belonging to a different category. Here the calculated density is greater than that found. This implies expansion on mixing. The values of γ are also greater, and together with these the values of $\gamma(Mv)^{\frac{2}{3}}$. But the rate of alteration of $\gamma(Mv)^{\frac{2}{3}}$ with temperature is practically normal, and the mean molecular weight can therefore be calculated with fair approach to accuracy. The fourth pair of liquids give still more abnormal results, owing probably to the fact that one of them has some power of association.

It would be premature to discuss these results without much more extended experimental evidence. Experiments have already been made with mixtures of alcohol and other liquids, and an investigation of mixtures of acetic acid is still in progress. The problem is a complex one; we have to deal with the extent to which the association of an associated liquid is altered by dilution; and it will form the subject of a further communication. We have, however, thought it advisable to bring forward some results in this paper to avoid the possible generalisation from the behaviour of the first two pairs of liquids that the molecular surface-energy of all liquids is the mean of those which they possess when unmixed.

One question remains to be considered; it is this: Is it justifiable to assume that the molecular surface-energy of a mixture of the associated and dissociated molecules of a substance whose molecular complexity alters with temperature is the mean of each taken singly? For, on that assumption, the mean molecular weights of associating liquids have been calculated. In our opinion it is; but, as direct experimental evidence is not as yet attainable, it may be well to bear in mind that, although a fair working hypothesis, it cannot be taken as a proved fact.

IV. "Flame Spectra at High Temperatures. Part II. The Spectrum of Metallic Manganese, of Alloys of Manganese, and of Compounds containing that Element." By W. N. HARTLEY, F.R.S. Received April 25, 1894.

(Abstract.)

The spectrum of manganese has been the subject of much investigation; the spark spectrum was examined by Huggins, Thalén, and Lecoq de Boisbaudran; the arc spectrum was studied by Ångström, Thalén, Cornu, Lockyer, also Liveing and Dewar; the flame spectra obtained from compounds of manganese were investigated by Simmler, Von Lichtenfels, Lecoq de Boisbaudran, and Lockyer, while Marshall Watts has given us accurate measurements of the wave-lengths of lines and bands observed in the spark and oxyhydrogen flame-spectra of spiegel-eisen, manganese dioxide, and other compounds of this metal.

When investigating the spectrum of the Bessemer flame, I found it necessary to compare the spectrum of elementary manganese under different conditions with that of its oxide when heated in the oxyhydrogen flame. Comparative experiments were made also with various alloys, as spiegel-eisen, silico-spiegel, ferromanganese, tool steel, and malleable nickel which contains manganese; also with compounds containing similar quantities of metal.

Metallic manganese was prepared by the electrolysis of manganese chloride, from which all other metals had been carefully separated. One preparation of pure manganese oxide was precipitated from a solution of potassium permanganate by the action of alcohol and a small quantity of sulphurous acid. Other specimens were precipitated from solutions of potassium permanganate by the addition of hydrogen peroxide. By this treatment pure manganic oxide containing only traces of potash was obtained. From one preparation even the potassium was removed.

Photographs of the spectra of metallic manganese and of manganic oxide were taken and compared. They were also compared with the spectra of the alloys of manganese. The periods of exposure varied from a mere flash in the case of spiegel-eisen when being poured into a Bessemer converter, to 30 minutes and even as much as 80 minutes with manganic oxide.

The leading features of the spectra of manganese and manganese oxide are the same, but they differ in detail, as may be observed by comparing the wave-lengths of the lines and bands in their respective spectra.

It will be readily understood that the bands can be measured with far less accuracy than lines, and that they are subject to some degree of variation in width, according to variation in the time of exposure and the temperature.

A striking group of lines, the most persistent in the whole of these spectra, is situated in the violet. The following measurements were made:—

4036·5	4034·9	Ångström, also Cornu.
4032·0	{4032·9, 4031·8}	Angström.
4029·5	4029·4	Ångström.

Another line is just visible about 4031·8, but it is so close to 4032·0 that it could be discerned only when the extreme points of three very strong lines were examined, and the plate was in perfect focus for that region. The whole group of lines appears as two bands very closely adjacent, or in the manganese oxide spectrum as one band with the centre appearing as if reversed, the less refrangible edge of the band being very strong and sharp, the more refrangible being degraded and diffuse. These lines remain after the bands in the yellow and green have disappeared from the photographs, but the result may be quite otherwise with eye observations, owing to the greater visibility of the yellow over the violet rays.

Photographs of the spectra obtained with a dispersion of four quartz prisms of 60°, and lenses of 15 inches in focal length, are presented with the paper.

V. "Flame Spectra at High Temperatures. Part III. The Spectroscopic Phenomena and Thermo-Chemistry of the Bessemer Process." By W. N. Hartley, F.R.S., Royal College of Science, Dublin. Received May 4, 1894.

(Abstract.)

The flame issuing from the mouth of a Bessemer converter was first investigated by Sir Henry Roscoe* in 1863; by Lielegg,† and by Marshall Watts in 1867;‡ by Tunner,§ J. M. Silliman, Rowan,|| Von

* 'Literary and Phil. Soc., Manchester, Proc.,' vol. 3, p. 57, and 'Phil. Mag.,' vol. 34, p. 437.

† 'Sitzungsberichte Kaiserl. Akademie der Wissenschaften,' Wien, vol. 56, Part II.

‡ 'Phil. Mag.,' vol. 34, p. 437.

§ 'Dingler's Polytech. J.,' vol. 178, p. 465.

|| 'Phil. Mag.,' vol. 41, p. 1.

Lichtenfels,* Spear Parker,† Kupelwieser,‡ Brunner,§ and Wedding in 1868;|| also by A. Greiner in 1874.¶

Up to the present time the precise nature of the spectrum, the cause of its production, its sudden disappearance when decarburization of the metal takes place, and the connexion between the decarburization of the metal and the extinction of the spectrum, have not been satisfactorily explained. According to Roscoe, Lielegg, Kupelwieser, and Spear Parker, the spectrum is characterised by bands of carbon or of carbon monoxide, which disappear when all carbon is burnt out of the metal.

On the other hand, according to the investigations of Simmler,** Brunner, Von Lichtenfels, and Wedding, the spectrum is not due to carbon (Roscoe) or to carbon monoxide (Lielegg and Kupelwieser), but to manganese and other elements in the pig-iron.

The very careful examination of these spectra by Watts and his comparison of them with that of the Bessemer flame led to the conclusion that it was not the spectrum of carbon in any form nor of manganese, but that of manganic oxide. Lielegg established the fact that carbon monoxide yields a continuous spectrum, and that this gas causes the continuous bright spectrum of the Bessemer flame; but he also attributed certain lines or bands to the high temperature of the carbon monoxide. All observers are agreed as to the appearance after a certain interval of the lines of the alkali metals which were originally discovered by Roscoe to be present during the first period of the "blow." Watts observed the C line of hydrogen during wet weather.

This research was undertaken in 1882, and an instrument was devised for the purpose of photographing the spectra of various flames emitted during metallurgical operations. The work was left in abeyance until certain practical difficulties encountered in studying flame spectra at high temperatures in the laboratory had been overcome. The original mounting of the instrument was too light, but that which has recently been used with success is described.

Owing to the courtesy of Mr. F. W. Webb, the engineer of the Locomotive Department of the London and North Western Railway, and of Mr. E. P. Martin, the manager of the Dowlais Ironworks, observations have been made at Crewe and at Dowlais during the past year. About ninety spectra were photographed, about fifty of which were available for study.

* 'Dingler's Polytech. J.,' vol. 191, p. 213.

† 'Chem. News,' vol. 23, p. 25.

‡ 'Oesterreichische Zeitschr. für Berg- und Hütten-Wesen,' No. 8, p. 59, 1868.

§ *Loc. cit.*, No. 29, p. 227, 1868.

|| 'Zeitschrift für das Berg- Hütten- und Salinen-Wesen,' vol. 27, p. 117, 1869.

¶ 'Revue Universelle,' vol. 35, p. 623.

** 'Zeitschr. für Analytische Chemie,' 1862.

The spectra studied extended from the red potassium line λ 7697, and on some of the plates to about the line P on Cornu's map of the solar spectrum, λ 3380·8; but the least refrangible line photographed was that of lithium λ 6707. The bands and lines in various spectra taken at Crewe and Dowlais have been measured, and their wave-lengths determined. Descriptions of the spectra and how they were obtained are given. The description of each band and line measured is given, with its wave-length, its origin, and other references. Photographs of the spectra are presented, and a map has been drawn for the identification of the lines on these photographs. About ninety-two lines were identified with lines in the solar spectrum, with lines in Kayser and Runge's map of the arc spectrum of iron, and on spectra from steel and ferric oxide heated in the oxyhydrogen flame.

The Constitution of the Bessemer Spectrum.

The spectrum is a complex one which exhibits differences in constitution during different periods of the "blow," and even during different intervals in the same period. As originally observed by Watts, the spectrum differs in different works, the difference being due to temperature and to the composition of the metal blown.

During the first period.	The lines of the alkali metals, sodium, potassium, and lithium, are seen unreversed on a bright continuous spectrum caused by carbon monoxide. The C line of hydrogen and apparently the F line were seen reversed during a snowstorm.
During the second period. The "boil."	Bands of manganese are prominent, overlying the continuous spectrum of carbon monoxide. There are lines of carbon monoxide, manganese, and iron, also those of the alkali metals.
During the third period. The "fining stage."	The spectrum is the same as the foregoing, but the lines of iron are not so strong and not quite so well defined. Some of the short lines disappear. The lines of the alkali metals are visible.

It is also probable that some of the bands of manganese oxide are present, but they are obscured by the continuous carbon monoxide spectrum. No absorption bands were seen, no nitrogen bands, nor bands of calcium and magnesium oxide, neither did the lines of these metals appear. There is no trace of cobalt, nickel, chromium, or copper; certain carbon bands overlie those of manganese, and are recognised by measurements of their edges. Some of the lines not identified by Watts prove to be iron lines, others belong to manganese. The manganese bands are all degraded towards the red, the carbon bands towards the blue.

The cause of the Non-appearance of Lines at the Commencement and Termination of the "Blow."

Some controversy followed upon the publication of the papers by Roscoe and Lielegg. Tunner stated that in Sweden the Bessemer process was not facilitated by the use of the spectroscope. Brunner pointed out that the spectroscopic phenomena were not dependent on the combustion of carbon, but were characteristic of the various impurities in the metal. Wedding and Silliman discussed the origin of the spectrum seen at different periods of the "blow," and failed to account fully at that time for the non-appearance of lines at its commencement and termination. Their views did not harmonise. Many facts were discovered which were not understood, appeared contradictory, and required verification. These have all been carefully examined and accounted for.

Support is given to Wedding's view, based on the analyses of Brunner, that the non-appearance of the lines of manganese at the commencement and termination of the blow is owing to the quantity of metal volatilised at those periods being insufficient for the production of a spectrum. At the commencement the temperature is too low, being very little above that of the molten metal; and, as free oxygen escapes along with carbon dioxide, the gaseous mixture contains too small a proportion of carbon monoxide. The alkalies which come from the ganister lining of the converter are present as silicates, and in very small proportion; many silicates, such as, for instance, felspar, do not exhibit spectra of the alkalies they contain until heated in the oxyhydrogen flame, but at this temperature the metals potassium, lithium, and rubidium have been detected with the greatest ease in such silicates. Similarly, the alkali metals do not show themselves in the Bessemer flame until a layer of slag has been formed, and the temperature has risen sufficiently high for these basic constituents to be vaporised. At the temperature of the "boil," or second period, both metallic manganese and iron are freely vaporised in a current of carbon monoxide, which, in a highly heated state, rushes out of the bath of molten metal. The evidence of this is the large number of bands of manganese and lines of iron in the spectrum.

When the metal blown contains but little manganese, as, for instance, hæmatite pig, this is all converted into silicate during the first period. The manganese spectrum in the flame does not arise from that substance being contained in the bath of metal, it must be vaporised from the slag. That this is so has been proved by photographs of the spectrum from samples of slag obtained from the Crewe works. There is very little difference between these and the photographs of the flame-spectrum taken at Crewe, during the "boil," the

difference being chiefly in the iron line being stronger in the slag spectrum. This explains the fact observed by Brunner, namely, that when a converter is being heated with coke after it has been used, but not re-lined, the spectrum of the Bessemer flame makes its appearance; manifestly it comes from the adhering slag.

The luminosity of the flame during the "boil" is due, not merely to the combustion of highly heated carbonic oxide, but also to the presence of the vapours of iron and manganese in the gas.

The disappearance of the manganese spectrum at the end of the "fining stage," or third period, is primarily due to a reduction in the quantity of heated carbon monoxide escaping from the converter, which arises from the diminished quantity of carbon in the metal. When the last traces of carbon are gone, so that air may escape through the metal, the blast instantly oxidises any manganese, either in the metal or in the atmosphere of the converter, and, furthermore, oxidises some of the iron. The temperature must then fall with great rapidity.

The entire spectroscopic phenomena of the "blow" are undoubtedly determined by the chemical composition of the molten iron, and of the gases and metallic vapours within the converter, the temperature of the metal and that of the issuing gases.

The Temperature of the Bessemer Flame.

The probable temperature of the Bessemer flame at the finish is that produced by the combustion in cold air of carbonic oxide heated to about 1580° C., that is to say, to the temperature which, according to Le Chatelier ('Comptes Rendus,' vol. 114, p. 670), is that of the bath of molten metal from which the gas has proceeded. The bath of metal acts simultaneously as a means of heating the blast, producing the gas, and as a furnace, on the regenerative principle, which heats the gas prior to its combustion. The heating effect is therefore cumulative. The temperature, as is well known, can easily rise too rapidly, and the metal has then to be cooled by throwing cold pig-iron, or even old ingot moulds, into it.

If we may judge by the lines and bands belonging to iron and manganese which have been measured in photographed spectra of the Bessemer flame, the temperature must nearly approach that of the oxyhydrogen flame, and may easily attain the melting point of platinum, namely, 1775° C. (Violle).

Marshall Watts observed ('Phil. Mag.,' 1870) that the sodium lines 5681 and 5687 may be employed as an index of temperature, since they are present in the spectrum of any flame containing sodium which is hot enough to melt platinum, but do not appear at lower temperatures. The Bessemer flame does not show this double line, but only the D lines.

We cannot, however, conclude from this that the flame is not hot enough to produce these lines, for though the temperature may be high enough the quantity of material present is not sufficient to cause their appearance. Moreover, there are two intensely brilliant bands of manganese closely adjacent, one of which certainly overlies these lines. Lastly, they are not to be seen in the photographed spectrum obtained from slag heated in the oxyhydrogen flame, which melts platinum easily and slowly volatilises iridium wire.

From thermo-chemical data the heat evolved during the "blow" has been calculated, but the specific heats of cast iron, slag, carbon monoxide, and nitrogen are unknown at temperatures between 1200° C. and 2000° C. If we allow for 50 per cent. of the heat developed at high temperatures being lost by radiation or absorbed, then the estimated temperature of the metal in the converter is more than 1900° C.

Le Chatelier ('Comptes Rendus,' vol. 114, p. 670) found the steel in the ladle of a Robert converter to be at 1640° C. Reasons are adduced for believing that it must certainly have been hotter than this at the highest temperature of the "blow."

The Technical Aspect of this Investigation.

The spectrum obtained from Bessemer-slag by the oxyhydrogen flame is composed of precisely the most characteristic features of the flame spectrum, as seen issuing from the converter at Crewe. Hence at this temperature iron and manganese are freely volatilised, as they are in the oxyhydrogen flame. As a matter of course the continuous spectrum of carbon monoxide, the bands and lines of that compound and of elementary carbon are absent from the slag spectrum. The flame spectrum at Dowlais differs from this, and resembles the spectrum of metallic manganese or more closely that of ferro-manganese. For reasons given, I conclude that the spectrum at Crewe results from materials in the slag; but that at Dowlais from constituents vaporised from the bath of metal.

The complete termination of the "fining stage" is clearly indicated, but there is no indication by the flame of the composition of the metal within the converter at any previous stage. As the progress of the "blow" is governed by the composition of the metal and its temperature in the converter, and as these cannot be controlled with perfect exactitude during each "blow," it follows that the practice of complete decarburization* is the best course to pursue, the required

* The words "carburizing" and "decarburizing" are to be preferred to "carbonising" and "decarbonising" when applied to metals, because these expressions were those originally used in the older works on metallurgy, and they avoid confusion with the other signification of the word "carbonising."

amount of carbon and manganese being added subsequently in the forms of grey iron, spiegel, or ferro-manganese.

I propose to continue this work by extending my observations to the flame from the basic Bessemer process and the gases in the Siemens steel furnace.

VI. "On a Method for determining the Thermal Conductivity of Metals, with Applications to Copper, Silver, Gold, and Platinum." By JAMES H. GRAY, M.A., B.Sc., 1851 Exhibition Scholar, Glasgow University. Communicated by LORD KELVIN, P.R.S. Received May 24, 1894.

(Abstract.)

The object of this investigation was to obtain a method for determining thermal conductivities of metals, which would not require either elaborate preparations or large quantities of the substances to be tested, and by means of which a test could be made in a few hours.

The method about to be described was suggested by Lord Kelvin thirty years ago, and is the experimental realisation of the theoretical conditions implied in the fundamental formula

$$Q = kA\frac{v-v_0}{l}t,$$

where the symbols have their usual meaning.

The apparatus was made so as to be suitable to test the metals in the form of wires of circular section.

The diameters found most convenient were from 2 to 4 mm., the lengths from 4 to 8 cm.

One end of a given length of the wire is kept at a constant known temperature. The rise of temperature of the other end of the wire is noted every minute, and, if proper precautions be taken to prevent loss by radiation from the sides, the data are obtained for calculating the thermal conductivity.

The wire to be tested is soldered at one end into the bottom of a copper box, 16 cm. long, 6 cm. wide, and 7 cm. deep. The bottom of the box is made of copper 3 mm. thick, the sides of thin sheet copper.

In the box, immediately above the hole into which the wire is soldered, there is a large block of copper, in which a hole has been made sufficiently large to admit a small thermometer.

The box is filled with water and supported at its middle by being fitted into an asbestos-lined wooden screen, 24 × 24 cm. The water is heated by a Bunsen burner placed on the other side of the screen

from that on which the wire is. No heat can therefore be communicated directly to the wire from the lamp. In the bottom of the box above the lamp a number of thick copper pins is fixed, so as to catch and distribute the heat. 3 mm. length of the other end of the wire is soldered into a solid copper ball, diameter 5·5 cm. In the ball a hole 3 cm. deep is made, so as to admit the bulb and part of the stem of a small and very sensitive thermometer. This thermometer is graduated from 5° C. to 20° C., and can be easily read to within one-fortieth of one degree. The bulb is surrounded by water.

To prevent radiation from the surface of the wire, a tube of circular section, diameter 1 cm., made of several layers of thin paper, surrounds the wire all along its length. The air inside this tube soon takes up the temperature of the part of the wire with which it is in contact, and so practically eliminates radiation.

A rough calculation gives for the maximum value of the loss due to radiation, 5·5 per cent. when the surface of the wire is exposed to the air, the length being 4 cm. Unless the paper tube is effective, the error due to radiation ought to be greater, the greater the length. Exhaustive trials, however, proved that different lengths gave practically the same value for the conductivity.

The other possible errors, besides radiation, to be tested for are:—

(1) The thermometer in the hot water may not indicate the temperature of the end of the wire.
(2) The solder may cause some error.
(3) The thermometer in the ball may not indicate the average temperature.
(4) There may be a lag in the thermometer.
(5) The temperature of the ball may not be the same throughout, and the thermometer may not indicate the temperature of the wire where it enters the ball.

All these errors are practically tested by using different lengths or diameters of the wire, and the results obtained in the present investigation indicate that the errors have been eliminated.

To test whether the thermometer in the hot water indicated the temperature at the end of the wire, a thermo-electric junction, made of very thin platinoid and copper wires, was soldered to the wire just where it entered the box. The other junction was tied to a thermometer and immersed in water, which was heated till there was no deflection in the sensitive mirror galvanometer which was used. The temperature indicated by the thermometer was then found to be the same as that of the thermometer in the hot water.

An approximate calculation for the other end of the wire shows that the temperature of that end is somewhat lower than that of the ball, the greatest difference being 1·5 per cent. This difference was

always allowed for by applying an approximate formula to each different length.

In order to make a complete test of a metal it is only necessary to take a wire of 5 or 6 cm. length and solder it firmly, the one end into the bottom of the heating box, the other into the calorimeter ball. The water in the heating box is kept boiling briskly, and readings are taken every half minute from the thermometer in the ball. These readings are then put upon a curve as ordinates, with the time in minutes as abscissæ. From this curve the rise of temperature per unit time can then be accurately read off, and, the thermal capacity of the ball being already determined, the flow of heat per unit time is obtained.

In order to eliminate radiation from the surface of the calorimeter ball, the latter is, at the beginning of the experiment, cooled to about 6° or 7° C. below the temperature of the air, or rather of the water-jacket which surrounds the ball.

Let α be the quantity of heat that passes from the surface of the ball, when the latter is $\theta°$ above or below the temperature of the water-jacket; Q_1 the quantity of heat that flows into the ball at the temperature $\theta°$ *above* that of the water-jacket; Q_2 the quantity that flows in when the ball is $\theta°$ below that of the jacket; T the temperature of the hot end of the wire.

Then if κ is the mean conductivity,

$$Q_1 = \kappa\,(T-\overline{t+\theta})-\alpha,$$

$$Q_2 = \kappa\,(T-\overline{t+\theta})+\alpha,$$

$$\therefore\ \tfrac{1}{2}\,(Q_1+Q_2) = \kappa\,(T-t).$$

If, therefore, the rise in temperature per half minute at $\theta°$ *above* that of the water-jacket be taken from the curve and added to the rise for $\theta°$ *below* the temperature of the jacket, the quantity $\frac{1}{2}(Q_1+Q_2)$ is obtained, and is the flow of heat when the temperatures of the ends of the wire are T° and $t°$ C., the radiation from the ball being thus eliminated. If ten or fifteen of these values be taken from the curve and the mean found, a very accurate result is obtained. It is thus immaterial whether the surface of the ball changes between each test, as long as it remains constant during the test.

The metal which was chiefly used for the exhaustive tests of the method was copper wire, of diameter 0·21 cm., density 8·85, volume specific (electrical) resistance at about 13° C. 1834 in absolute units.

The number obtained for the absolute value of the thermal conductivity was 0·88838 C.G.S. units, which was the mean of the values for different lengths of from 4 to 7 cm., the greatest variation being a little over 1 per cent.

The greatest value obtained for copper was 0·9594 C.G.S. units, which was for wire obtained from Messrs. Glover and Sons. The specific (electrical) resistance was found to be 1730.

It must be noted that these values are the means of the conductivities corresponding to the temperatures at the ends of the wire. When compared with the values obtained by other experimenters, the results of the latter must be taken for the mean of 97° C. and 10° C., that is 53° C.

For this temperature Ångström gives 0·9208.

Several qualities of copper were tested, as well as pure gold, silver, and platinum, kindly lent for the investigation by Messrs. Johnson, Matthey, and Co.

The values are given below:—

Mean Conductivity between Temperatures 10° C. and 97° C.

	Thermal conductivity in C.G.S. units.	Diameter.
Copper, Specimen 1	0·9594	2·00 mm.
" " 2	0·88838	2·11 "
" " 3	0·8612	3·09 "
" " 4 (very impure)	0·3497	2·04 "
" " 5 "	0·3198	2·04 "
Silver (pure)	0·9628	2·02 "
Gold "	0·7464	2·00 "
Platinum "	0·1861	2·00 "

Experiments to find out if there is any relation between the electrical and thermal conductivities confirmed what has been found by previous investigators, that if one metal is a better conductor for heat it is also a better conductor for electricity. The results did not, however, prove that the ratios were always the same, although in some cases they agreed very closely.

For example—

$$\frac{\text{Conductivity of Specimen 2 in above table}}{\text{Conductivity of Specimen 5 in above table}} = 2{\cdot}78 \text{ for heat.}$$

$$= 2{\cdot}86 \text{ for electricity.}$$

$$\frac{\text{Conductivity of Specimen 2}}{\text{Conductivity of Specimen 4}} = 2{\cdot}54 \text{ for heat.}$$

$$= 2{\cdot}56 \text{ for electricity.}$$

$$\frac{\text{Conductivity of Specimen 1}}{\text{Conductivity of Specimen 2}} = 1{\cdot}08 \text{ for heat.}$$

$$= 1{\cdot}066 \text{ for electricity.}$$

While, however, these numbers agree very closely, other wires were tested in which the numbers varied considerably.

It is intended to go on with tests of alloys, such as platinoid and German silver; also, by using liquids other than water, to obtain values of the variation of conductivity with temperature.

Presents, June 14, 1894.

Transactions.

Berlin:—Gesellschaft für Erdkunde. Verhandlungen. Bd. XXI. No. 5. 8vo. *Berlin* 1894. The Society.

Bucharest:—Societatea de Sciințe Fizice. Buletinul. Anul III. No. 1—2. 8vo. *Bucurescĭ* 1894. The Society.

Calcutta:—Indian Museum. Notes. Vol. III. No. 3. 8vo. *Calcutta* 1894. The Museum.

Kew:—Royal Gardens. Bulletin of Miscellaneous Information. 1894. Appendix 2. 8vo. *London.* The Director.

Liége:—Société Géologique de Belgique. Annales. Tome XXI. Livr. 2. 8vo. *Liége* 1893–94. The Society.

London:—Anthropological Institute. Journal. Vol. XXIII. No. 4. 8vo. *London* 1894. The Institute.

East India Association. Journal. Vol. XXVI. No. 4. 8vo. *London* 1894. The Association.

Entomological Society. Transactions. 1894. Part 2. 8vo. *London.* The Society.

Manchester:—Geological Society. Transactions. Vol. XXII. Parts 16—18. 8vo. *Manchester* 1894. The Society.

Meriden:—Meriden Scientific Association. Transactions. Vol. V. 8vo. *Meriden, Conn.* 1894. The Association.

Milan:—Società Italiana di Scienze Naturali. Atti. Vol. XXXIV. Fasc. 4. 8vo. *Milano* 1894. The Society.

Moscow:—Société Impériale des Naturalistes. Bulletin. Année 1893. No. 4. 8vo. *Moscou* 1894. The Society.

Philadelphia:—American Philosophical Society. Proceedings. Vol. XXXI. No. 142. 8vo. *Philadelphia* 1893. The Society.

Rio de Janeiro:—Museu Nacional. Archivos. Vol. VIII. 4to. *Rio de Janeiro* 1892. The Museum.

Washington:—Smithsonian Institution. Contributions to Knowledge. Vol. XXVII. No. 884. 4to. *Washington* 1893. The Institution.

United States National Museum. Bulletin. Nos. 44—46. 8vo. *Washington* 1893; Proceedings. Vol. XV. 8vo. *Washington* 1893; Report for the year ending June 30, 1891. 8vo. *Washington* 1892. The Museum.

Observations and Reports.

India :—Archæological Survey of India. The Bower Manuscript. Edited by A. F. R. Hoernle. Part 2. Fasc. 1. 4to. *Calcutta* 1894. The Survey.

Madison :—Washburn Observatory of the University of Wisconsin. Publications. Vol. VIII. 8vo. *Madison, Wis.* 1893. The Observatory.

Mauritius :—Royal Alfred Observatory. Annual Report of the Director. 1892. Folio. [*Mauritius* 1894.] The Observatory.

Sydney :—Department of Public Works. Report. 1892. Folio. *Sydney* 1893. The Department.

Washington :—United States Commission of Fish and Fisheries. Report of the Commissioner. Part XVII. 8vo. *Washington* 1893. The Commission.

United States Department of Agriculture. Report of the Chief of the Weather Bureau. 1891–92. 4to. *Washington* 1893. The Department.

Journals.

Horological Journal. Vol. XXXVI. No. 430. 8vo. *London* 1894. British Horological Institute.

Medico-Legal Journal. Vol. XI. No. 3. 8vo. *New York* 1893. The Editor.

Nature Notes. Vol. V. No. 54. 8vo. *London* 1894. Selborne Society.

Scientific Memoirs by Medical Officers of the Army of India. Part 8. 4to. *Calcutta* 1894. India Office.

Debus (H.), F.R.S. Ueber einige Fundamental-Sätze der Chemie insbesondere das Dalton-Avogadro'sche Gesetz. 8vo. *Cassel* 1894. The Author.

Garcia de la Cruz (V.). Lois Mécaniques des Liquides Troubles et des Gaz Nébuleux. 8vo. *Barcelone* 1894. The Author.

June 21, 1894.

The LORD KELVIN, D.C.L., LL.D., President, in the Chair.

Dr. John Rose Bradford and Professor M. J. M. Hill were admitted into the Society.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Researches on Explosives. Preliminary Note." By Captain Sir A. NOBLE, K.C.B., F.R.S., M.I.C.E., &c. Received June 13, 1894.

The researches on which I, in conjunction with Sir F. Abel, have been engaged for very many years, have had their scope so altered and extended by the rapid advances which have been made in the science of explosives, that we have been unable to lay before the Society the results of the many hundreds of experiments under varied conditions which I have carried out. We are desirous also of clearing up some difficulties which have presented themselves with certain modern explosives when dealing with high densities and pressures, but the necessary investigations have occupied so much time that I am induced to lay a few of our results before the Society, trusting, however, that before long we may be able to submit a more complete memoir.

A portion of our researches includes investigations into the transformation and ballistic properties of powders varying greatly in composition, but of which potassium nitrate is the chief constituent. In this preliminary note I propose to refer to powders of this description chiefly for purposes of comparison, and shall devote my attention principally to gun-cotton and to those modern explosives of which gun-cotton forms a principal ingredient.

In determining the transformation experienced during explosion, the same arrangements for firing the explosive and collecting the gases were followed as are described in our earlier researches,* and the gases themselves were, after being sealed, analysed either under the personal superintendence of Sir F. Abel, or of Professor Dewar,

* 'Phil. Trans.,' vol. 165, p. 61.

and to Professor Dewar's advice and assistance I am indebted, I can hardly say to what extent.

The heat developed by explosion, and the quantity of permanent gases generated were also determined as described in our researches, but the amount of water formed plays so important a part in the transformation that special means were adopted in order to obtain this product with exactness.

The arrangement employed was as follows :—

After explosion the gases formed were allowed to escape through two U-tubes filled with pumice stone and concentrated sulphuric acid; when the gases had all escaped the explosion cylinder was opened, and the water deposited at the bottom of the cylinder was collected in a sponge, placed in a closed glass vessel and weighed. The cylinder was then nearly closed and heated, and a measured quantity of air was, by means of an aspirator, drawn slowly through the U-tubes till the cylinder was perfectly dry. This was easily ascertained by observing when moisture was no longer deposited on a cooled glass tube through which the air passed.

The U-tubes were then carefully weighed, the amount of moisture absorbed determined, and added to the quantity of water directly collected. The aqueous vapour in the air employed for drying was, for each experiment, determined and deducted from the gross amount.

Numerous experiments were made to ascertain the relation of the tension of the various explosives employed, to the gravimetric density of the charge when fired in a close vessel, but I do not propose here to pursue this part of our enquiry, both because the subject is too large to be treated of in a preliminary note and because approximate values have already been published* for several of the explosives with which we have experimented.

With certain explosives, the possibility or probability of detonation was very carefully investigated. In some cases the explosive was merely placed in the explosion vessel in close proximity to a charge of mercuric fulminate by which it was fired, but I found that the most satisfactory method of experiment was to place the charge to be experimented with in a small shell packed as tightly as possible, the shell then being placed in a large explosion vessel and fired by means of mercuric fulminate. The tension in the small shell at the moment of fracture and the tension in the large explosion vessel were in each experiment, carefully measured.

It may be desirable here to explain that I do not consider the presence of a high pressure with any explosive as necessarily denoting detonation. With both cordite and gun-cotton I have developed enormous pressures, close upon 100 tons per square inch (about

* Noble, 'Internal Ballistics,' 1892, p. 33; 'Roy. Soc. Proc.,' vol. 52, p. 123.

15,000 atmospheres), but the former explosive I have not succeeded in detonating, while gun-cotton can be detonated with the utmost ease. It is obvious that if we suppose a small charge fired in a vessel impervious to heat, the rapidity or slowness of combustion will make no difference in the developed pressure, and that pressure will be the highest of which the explosive is capable, regard being of course had to the density of the charge. I say a small charge, because, if a large charge were in question and explosion took place with extreme rapidity, the nascent gases may give rise to such whirlwinds of pressure, if I may use the term, that any means we may have of registering the tension will show pressures very much higher than would be registered were the gases, at the same temperature, in a state of quiescence. I have had innumerable proofs of this action, but it is evident that in a very small charge the nascent gases will have much less energy than in the case of a large charge occupying a considerable space.

The great increase in the magnitude of the charges fired from modern guns has rendered the question of erosion one of great importance. Few, who have not had actual experience, have any idea how rapidly with very large charges the surface of the bore is removed. Great attention has therefore been paid to this point, both in regard to the erosive power of different explosives and in regard to the capacity of different materials (chiefly different natures of steel) to resist the erosive action.

The method I adopted for this purpose consisted in allowing large charges to escape through a small vent. The amount of the metal removed by the passage of the products of explosion, which amount was determined by calibration, was taken as a measure of the erosive power of the explosive.

Experiments have also been made to determine the rate at which the products of explosion part with their heat to the surrounding envelope, the products of explosion being altogether confined. I shall only briefly allude to these experiments, as, although highly interesting, they have not been carried far enough to entitle me to speak with confidence as to final conclusions.

Turning now to ballistic results. The energies which the new explosives are capable of developing, and the high pressures at which the resulting gases are discharged from the muzzle of the gun, render length of bore of increased importance. With the object of ascertaining with more precision the advantages to be gained by length, the firm to which I belong has experimented with a 6-inch gun of 100 calibres in length. In the particular experiments to which I refer, the velocity and energy generated has not only been measured at the muzzle, but the velocity and the pressure producing this velocity have been obtained for every point of the bore, consequently

the loss of velocity and energy due to any particular shortening of the bore can be at once deduced.

These results have been obtained by measuring the velocities every round at sixteen points in the bore and at the muzzle. These data enable a velocity curve to be laid down, while from this curve the corresponding pressure curve can be calculated. The maximum chamber pressure obtained by these means is corroborated by simultaneous observations taken with crusher gauges, and the internal ballistics of various explosives have thus been completely determined.

Commencing with gun-cotton, with which a very large number of analyses were made, with the view of determining whether there was any material difference in the decomposition dependent upon the pressure under which it was exploded, two descriptions were employed: one in the form of hank or strand, and the other in the form of compressed pellets. Both natures were approximately of the same composition, of Waltham Abbey manufacture, containing in a dried sample about 4·4 per cent. of soluble cotton and 95·6 per cent. of insoluble. As used, it contained about 2·25 per cent. of moisture.

The following were the results of the analyses of the permanent gases. They are placed in five series, viz.:—

First. Analyses showing the decomposition of the strand or hank gun-cotton. Second. Analyses showing the decomposition of pellet gun-cotton.

In both these series the analyses are arranged in the order of the ascending pressures under which the decomposition took place.

Third and fourth. Examples of the decomposition of strand and pellet gun-cotton when exploded by means of mercuric fulminate; and, fifth, a series showing the decomposition experienced by pellet gun-cotton saturated with from 25 to 30 per cent. of water, and detonated by means of a primer of dry gun-cotton and mercuric fulminate.

I leave these results for discussion in the memoir which Sir F. Abel and I hope before long to submit, and will only remark that, in Tables I and II, the same peculiarity we have before remarked upon in reference to gunpowder, is again exhibited; I mean the marked manner in which the carbonic anhydride increases with the pressure. It will be noted that in Table I the volumes of carbonic anhydride and carbonic oxide are nearly exactly reversed; again, considering that the composition of the pellet and strand gun-cotton is practically the same, the distinct difference between the proportions of these products in the two series is sufficiently remarkable. It not improbably is connected with the rapidity of combustion of the two samples. Another striking peculiarity is the manner in which the CO_2 is increased (as exhibited in Table V) when saturated pellet cotton is detonated.

I.—Results in Volumes of the Analyses of the Permanent Gases generated by the Explosion of Strand Gun-cotton, arranged according to ascending Pressures.

Under pressure of gas.	Tons per square inch.										
	1·5	2·5	8·0	8·0	12·0	12·3	18·0	20·0	45·0 ?	48·0 ?	50·0 ?
CO_2 ...... (vols.)	26·49	29·62	30·95	31·00	32·23	32·70	33·63	33·01	34·70	34·77	36·18
CO ...... „	36·66	35·03	32·27	32·76	30·65	31·36	31·20	30·32	28·60	28·66	27·57
H .... „	19·68	17·13	19·10	18·80	20·38	19·23	17·99	18·25	16·56	17·48	16·76
N .... „	16·85	18·18	17·20	16·90	16·43	16·25	16·23	16·60	16·83	16·05	16·15
CH_4 ...... „	0·32	0·04	0·48	0·54	0·31	0·46	0·95	1·82	3·31	3·04	3·34

II.—Similar Analyses for Pellet Gun-cotton.

Under pressure of gas.	Tons per square inch.									
	1·0	1·5	6·5	11·0	14·0	15·0	17·0	17·0	25·0	30·0
CO_2 ...... (vols.)	21·50	25·03	25·61	26·68	27·41	25·75	28·54	28·39	28·24	28·88
CO ...... „	39·70	36·85	39·51	36·97	37·23	38·00	35·52	36·41	34·94	35·64
H .... „	22·83	21·00	18·80	19·59	19·37	19·71	18·47	19·64	20·30	20·50
N .... „	15·58	15·88	15·97	15·91	15·35	15·26	16·08	14·90	15·59	14·98
CH_4 ...... „	0·39	1·24	0·11	0·85	0·64	1·28	1·39	0 66	0·93	

III. Results of the Analyses of Strand Gun-cotton when fired in a Close Vessel by Detonation.

	Pressure* per sq. inch.	
	1 ton.	3 tons.
CO_2 (vols.)	19·21	29·08
CO „	41·25	32·88
H „	23·07	20·14
N „	16·21	17·50
CH_4 „	0·26	0·75

IV. Similar Results for Pellet Gun-cotton.

	Pressure per sq. inch.	
	3 tons.	10 tons.
CO_2 (vols.)	25·76	26·50
CO „	39·34	37·48
H „	18·71	20·97
N „	16·19	15·05
CH_4 „	Nil	Nil

V. Results of Analyses of Saturated Pellet Gun-cotton fired in a Close Vessel by Detonation.

	Pressure per square inch			
	Under 10 tons.	10·5 tons.	16 tons.	16·5 tons.
CO_2 (vols.)	32·14	33·25	32·93	35·60
CO „	27·04	25·90	27·25	23·43
H „	26·80	26·53	25·76	24·22
N „	13·83	14·32	14·06	15·25
CH_4 „	0·19	Nil	Nil	1·50

Such are the average analyses of the permanent gases generated by the decomposition of gun-cotton under the various conditions I have described, and it will be evident from these analyses that the volumes of the permanent gases may be expected to differ to some very appreciable extent, depending both upon the density under which it is exploded, and also upon the mode of explosion. I have found it most convenient to explode the charges, the permanent gases from which were to be measured, under a pressure of about 10 tons per square inch (1,524 atmospheres), and, under these circumstances, the average of several very accordant determinations gave, at 0° C. and 760 mm. of mercury, 689 c.c. per gram of strand gun-cotton and 725 c.c. per gram of pellet gun-cotton.

* The pressures given are those due to the gravimetric density of the charge.

At the temperature of explosion the whole of the water formed is in the gaseous state. It is therefore necessary, in order to obtain the total gaseous volume, to add to the above volumes of permanent gases the equivalent volume of aqueous vapour at the temperature and pressure stated. Now the quantity of water formed by the explosion of 129·6 grams of gun-cotton was found to be 16·985 grams; hence 1 gram of gun-cotton generated 0·1311 gram of water, equivalent to 162·6 c.c. of aqueous vapour, and the total volume of gaseous matter at the temperature and pressure stated is for strand gun-cotton 852·2 c.c. per gram, for pellet 887·6 c.c.

The heat measured reached, with strand gun-cotton, 1068 gram-units water fluid, or 988 gram-units water gaseous, while with pellet gun-cotton these figures were 1037 or 957 gram-units respectively.

Pellet gun-cotton made at Stowmarket generated 738 c.c. of permanent gas and 994 units of heat per gram, while dinitro-cellulose containing 12·8 per cent. of nitrogen generated 748 c.c. of gas and 977 units of heat, the water in both cases being fluid.

Gun-cotton, both pellet and strand, I have detonated by means of mercuric fulminate with ease and certainty. The effect of employing this means of ignition in a close vessel is very striking, and the indications of intense heat are much more apparent than when the charge is fired in the ordinary way. This effect is probably partly due to an actual higher temperature, caused by the greater rapidity of combustion. I allude elsewhere to the extreme rapidity with which the gases part with their heat, but this higher heat is, I think, clearly indicated by the surfaces of the internal crusher gauges becoming covered with innumerable small cracks and by thin laminæ occasionally flaking off exposed surfaces; but perhaps the most striking proof of the violence of this detonation is shown by its action on a cast-iron shell fired as I have described; where no detonation takes place the shell is broken into fragments of various sizes, such as are familiar to all acquainted with the bursting of shell; but when detonation, with gun-cotton, for example, takes place, the whole shell is reduced to very minute fragments, and, what is more remarkable, two-thirds of the total weight are generally in the form of small peas and of the finest dust.

The ease with which gun-cotton can be detonated renders it unsuitable for use as a propulsive agent unless this property be in some way neutralised. I have, therefore, made but few experiments in this direction, and shall not further allude to them in this note, as more suitable explosives, explosives also of which gun-cotton is a principal component, have been elaborated, and these not only possess to the full the high ballistic properties of gun-cotton, but are more or less free from the tendency to detonate, which, however useful it may be

in other directions, is a fatal objection to the employment of gun-cotton for propelling purposes.

Turning now to cordite; cordite consists, as is well known, of nitro-glycerine and gun-cotton as its main ingredients. As now made it contains 37 per cent. of gun-cotton (trinitro-cellulose with a small proportion of soluble gun-cotton), 58 per cent. of nitro-glycerine, and 5 per cent. of a hydrocarbon known as vaselin. On account of the importance of this explosive, I have made numerous experiments, both with large and small charges, to determine the relation of the tension to the density of the charge. Up to densities of 0·55 the relation may be considered to be very approximately determined; above that density, although many determinations have been made, these determinations have shown such wide variations that they cannot, until certain discrepancies are explained, be assumed as at all accurate.

The average results of some of the analyses of the permanent gases are given below:—

The first four analyses were made from experiments with the earlier samples of cordite when tannin formed an ingredient of cordite. They are not, therefore, strictly comparable with the later analyses. There appears also to be a difference in the transformation, slight but decided, which the same cordite experiences, dependent upon the diameter of the cord, and this difference is shown at once in the analyses, in the volume of permanent gases, in the heat developed, and, I think, in the amount of aqueous vapour formed.

The following are some of the analyses:—

VI.

	Pressure per square inch.							
	0·048 Cordite.				0·255 Cordite.			
	2·5 tons.	6 tons.	10 tons.	14 tons.	10 tons.	12 tons.	11 tons.	14 tons.
CO_2	29·9	30·4	32·0	31·6	27·0	28·4	23·9	26·3
CO......	28·3	30·7	32·9	32·1	34·2	33·8	37·2	35·8
H	19·3	20·0	18·0	21·6	26·9	24·4	28·4	26·1
N	22·5	18·9	17·1	14·8	12·0	13·4	10·4	11·8
CH_4	traces.							

In the whole of these analyses the water formed by the explosion smelt strongly of ammonia.

The quantity of permanent gases measured, under the same conditions as in the case of gun-cotton, was found to be—

For the earlier cordite, 655 vols.

For the present service cordite, 0·255 in. in diameter, 692 vols., and for that 0·048 in. in diameter, 698 vols. In the two latter samples the aqueous vapour was determined, and was found to

amount to 20·257 grams for the 0·255-in. cordite, and to 20·126 grams for the 0·048-in. cordite; or, stating the result per gram, these figures are respectively equivalent to 0·1563 gram, or 194 c.c. aqueous vapour, and to 0·1553 gram, or 192·5 c.c. per gram of cordite.

Hence the total gaseous products generated by the explosion of cordite amount per gram to 886 c.c. for the 0·255-in. cordite, and to 890·5 c.c. for the 0·048-in. cordite, the volumes being, of course, taken at 0° C. and 760 mm. atmospheric pressure.

The heat generated was found to be:—For the earlier cordite, 1214 gram-units water fluid; for the service 0·255-in. cordite, 1284 gram-units water fluid or 1189 units water gaseous; for the service 0·048-in. cordite, 1272 units water fluid or 1178 units water gaseous.

From my very numerous experiments on erosion I have arrived at the conclusion that the principal factors determining its amount are: (1) the actual temperature of the products of combustion, (2) the motion of these products. But little erosive effect is produced, even by the most erosive powders, in close vessels, or in those portions of the chambers of guns where the motion of the gas is feeble or *nil;* but the case is widely different where there is rapid motion of the gases at high densities. It is not difficult absolutely to retain without leakage the products of explosions at very high pressures, but if there be any appreciable escape before the gases are cooled they instantly cut a way for themselves with astonishing rapidity, totally destroying the surfaces over or through which they pass. Among all the explosives with which I have experimented I have found that where the heat developed is low the erosive effect is also low.

With ordinary powders, the most erosive with which I am acquainted is that which, on account of other properties, is used for the battering charges of heavy guns: I refer to brown prismatic powder. The erosive effect of cordite, if considered in relation to the energy generated by the two explosives, is very slightly greater than that of brown prismatic, but very much higher effects can, if it be so desired, be obtained with cordite, and, if the highest energy be demanded, the erosion will be proportionally greater. There is, however, one curious and satisfactory peculiarity connected with erosion by cordite. Erosion produced by ordinary gunpowder has the most singular effect on the metal of the gun, eating out large holes and forming long rough grooves, resembling a ploughed field in miniature, and these grooves have, moreover, the unpleasant habit of being very apt to develop into cracks; but with cordite, so far as my experience goes, the erosion is of a very different character. The eddy holes and long grooves are absent, and the erosion appears to consist in a simple washing away of the surface of the steel barrel.

Cordite does not detonate; at least, although I have made far more experiments on detonation with this explosive than with any other,

I have never succeeded in detonating it. With an explosive like cordite, capable of developing enormous pressures, it is, of course, easy, if the cordite be finely comminuted, to develop very high tensions, but, as I have already explained, a high pressure does not necessarily imply detonation.

The rapidity with which cordite gases lose their temperature, and consequently their pressure, by communication of their heat to their surrounding envelope is very striking. Exploding a charge of about 1¾ lbs. of cordite in a close vessel at a tension of a little over 6 tons on the square inch, or say 1000 atmospheres, I have found that the pressure of 6 tons per square inch was again reached in 0·07 sec. after explosion, of 5 tons in 0·171 sec., of 4 tons in 0·731 sec., of 3 tons in 1·764 secs., of 2 tons in 3·523 secs., and of 1 ton in 7·08 secs. The loss of pressure after 1 ton per square inch was reached was, of course, slow, but the figures I have given were closely approximated to in two subsequent experiments. With ordinary gunpowder the reduction of pressure was very much slower, as was to be expected, on account of the charge being much larger; on account, also, of the temperature of explosion being much lower.

These experiments are now being continued with larger charges and higher pressures.

It only remains to give particulars as to ballistics, that is as to the velocities and energies realisable by cordite in the bore of a gun, but these will be most conveniently given with similar details regarding other explosives with which I have experimented.

The ballistite I have used has, like the cordite, been changed in composition since the commencement of my experiments. The sample I used for my earlier experiments was nearly exactly composed of 50 per cent. of dinitro-cellulose (collodion cotton) and 50 per cent. of nitro-glycerine. The cubes were coated with graphite, and the nitro-cellulose was wholly soluble in ether alcohol.

The second sample was nominally composed of 60 per cent. of nitro-cellulose and 40 per cent. of nitro-glycerine. The proximate analysis gave

Nitro-glycerine	41·62
Nitro-cellulose	59·05

as before the whole of the nitro-cellulose was soluble in ether alcohol.

The earlier sample gave the following permanent gases under pressures of six and twelve tons per square inch respectively.

CO_2	37·3	38·49
CO	27·8	28·35
H	19·1	19·83
N	15·8	13·32
CH_4	traces.	

One gram of this ballistite gives rise to 610 c.c. of permanent gases, and to 0·1588 gram of aqueous vapour corresponding to 197 c.c. at 0° C and 760 mm.

Hence the total volume of gas is 807 c.c., and the heat generated by the explosion is 1,365 gram-units (water fluid), 1,269 gram-units (water gaseous).

Although I have not made nearly so many experiments on detonation with ballistite as with cordite, those I have made with the earlier samples (50 per cent. gun-cotton and 50 per cent. nitroglycerine) neither detonated, nor did they show any tendency to detonate, but the case is different with respect to a sample of ballistite consisting of 60 per cent. gun-cotton and 40 per cent. nitroglycerine. This sample, 0·2-in. cubes, detonated with great violence on two occasions, but I am unable, without further experience, to say whether this result was due to the change in the composition of the ballistite or to defective manufacture.

The erosive action of ballistite is, as might perhaps be anticipated from the higher heat developed, greater than with cordite, but the remarks made with respect to the action of cordite apply also to ballistite.

The French B.N. powder consists of nitro-cellulose partially gelatinised and mixed with tannin, with barium and potassium nitrates.

When exploded under a pressure of six tons per square inch the permanent gases were found to consist of

CO_2	28·1 vols.
CO	32·4 „
H	21·9 „
N	16·8 „
CH_4	0·8 vol.

These permanent gases occupied at the usual temperature and pressure a volume of 616 c.c.; the aqueous vapour formed occupied in addition 206 c.c., so that the total gaseous volume was 822 c.c.

The heat generated was 1,003 gram-units (water fluid) or 902 gram-units (water gaseous); the ballistics obtained with this powder are given along with those furnished by other explosives.

For purposes of comparison I have introduced among the ballistic results those obtained with amide prismatic powder, and with R.L.G. Particulars as to both these powders have already been given* and need not here be repeated.

In a preliminary note like the present, the most convenient mode

* 'Roy. Soc. Proc.,' vol. 52, p. 125; 'Phil. Trans.,' Part I, 1880, p. 278.

of comparing the velocities and energies developed by the new explosives is by the aid of diagrams.

Accordingly, in Fig. 1, I show the velocities of seven different explosives from the commencement of motion to the muzzle of the gun; the position of the points at which the velocity is determined are shown, and on the lowest and highest curves the observed velocities are marked where it is possible to do so without confusing the diagram. Lines are drawn to indicate the velocities that are obtained with the lengths of 40, 50, 75, and 100 calibres.

Fig. 2 shows the pressures by which the velocities of Fig. 1 were obtained. The areas of these curves represent the energies realised, and the lines intersecting the curves indicate the pressures at which the gases are discharged from the muzzle for lengths of 40, 50, 75, and 100 calibres respectively. The chamber pressures indicated by crusher gauges are also shown in Fig. 2, and it will be observed that the two modes of determining the maximum pressure are in general in close accordance.

It will further be observed that with the slow-burning powders the chronoscopic maximum pressures are somewhat, though not greatly higher, than are those indicated by the crusher gauges. This observation is not new.* It was noted in the long series of experiments with black powders carried on by the Committee of Explosives.

The result is widely different where an explosive powder or a quickly-burning powder, such as R.L.G., giving rise to wave-pressure is employed; the crusher gauge in such cases† gives considerably and frequently very greatly higher pressures, and this peculiarity is illustrated in the curve from R.L.G. in Fig. 2.

It is, perhaps, hardly necessary to point out that the results given in Fig. 1 have to be considered in relation to the facts disclosed in Fig. 2. Thus it will be noted that the velocities and energies realised by 22 lb. of 0·35-in. cordite and 20 lb. of 0·3-in. cordite are practically the same, but reference to Fig. 2 shows that with the 0·3-in. cordite this velocity and energy has been obtained at the cost of nearly 30 per cent. higher maximum pressure.

A similar remark may be made in regard to the French B.N. powder if compared with the ballistite. Its velocity and energy are obtained at a high cost of maximum pressure, and it is interesting to note how the velocity curve of B.N., which for the first four feet of motion shows a velocity higher than that of any other explosive, successively crosses other curves, and gives at the muzzle a velocity of 500 f.s. under that of cordite.

The velocities and energies at the principal points indicated in

* Noble and Abel, 'Phil. Trans.,' vol. 165, p. 110.

† Compare Noble and Abel, *loc. cit.*, p. 109.

Fig. 1.

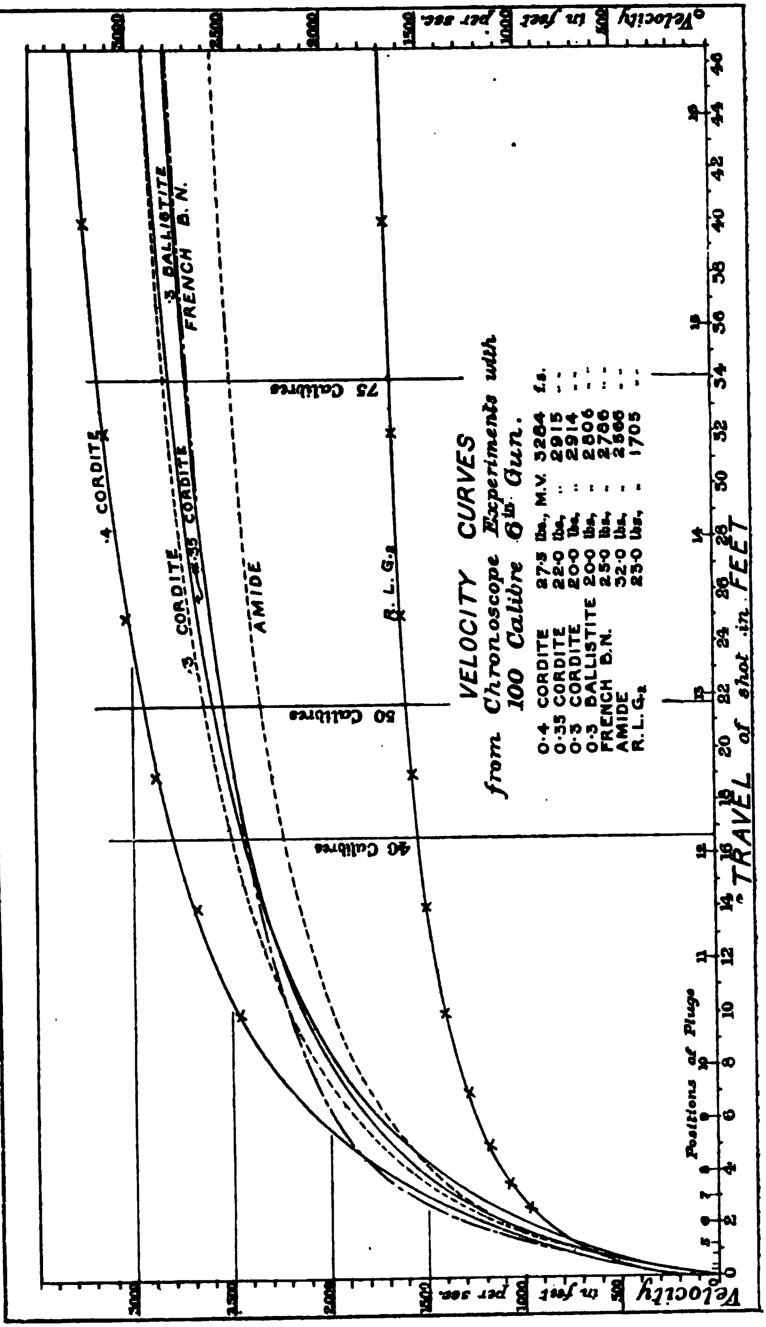
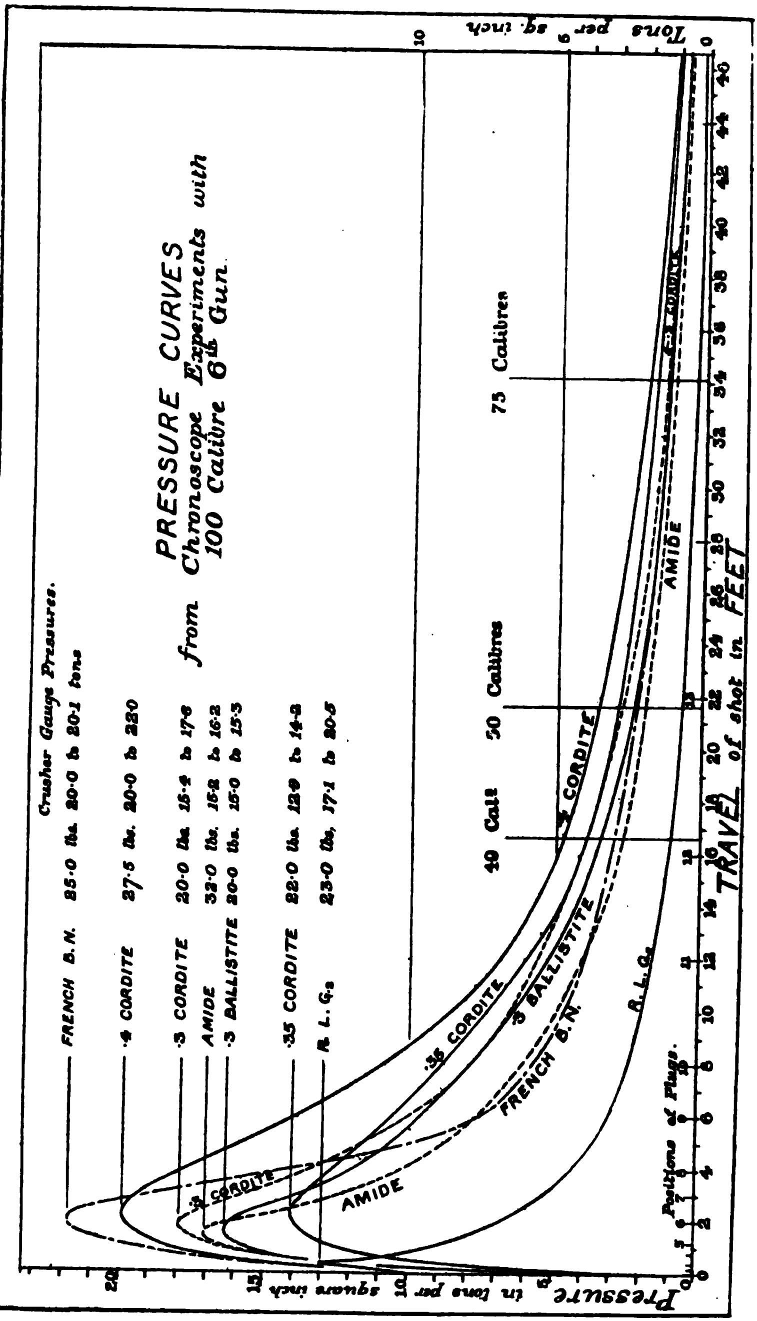

VELOCITY CURVES
from Chronoscope Experiments with
100 Calibre 6lb Gun.
0·4 CORDITE 27·5 lbs., M.V. 3284 f.s.
0·35 CORDITE 22·0 lbs., 2915
0·3 CORDITE 20·0 lbs., 2914
0·3 BALLISTITE 20·0 lbs., 2806
FRENCH B.N. 25·0 lbs., 2786
AMIDE 32·0 lbs., 2566
R.L.G.2 25·0 lbs., 1705
·4 CORDITE
·35 CORDITE
·3 CORDITE
·3 BALLISTITE
FRENCH B.N.
AMIDE
R.L.G.2
40 Calibres
50 Calibres
75 Calibres
Velocity in feet per sec.
TRAVEL of shot in FEET
Positions of Plugs

Fig. 2.


PRESSURE CURVES
from Chronoscope Experiments with
100 Calibre 6 in. Gun.
Crusher Gauge Pressures.
FRENCH B.N. 35·0 lbs. 20·0 to 20·1 tons
·4 CORDITE 27·5 lbs. 20·0 to 22·0
·3 CORDITE 20·0 lbs. 15·4 to 17·6
AMIDE 32·0 lbs. 15·2 to 16·2
·3 BALLISTITE 20·0 lbs. 15·0 to 15·3
·35 CORDITE 22·0 lbs. 12·9 to 14·2
R. L. G.₂ 23·0 lbs. 17·1 to 20·5
40 Cal.
50 Calibres
75 Calibres
·4 CORDITE
·3 CORDITE
·35 CORDITE
·3 BALLISTITE
FRENCH B.N.
R. L. G₂
AMIDE
Pressure in tons per square inch
Tons per sq. inch
TRAVEL of shot in FEET
Positions of Plugs.

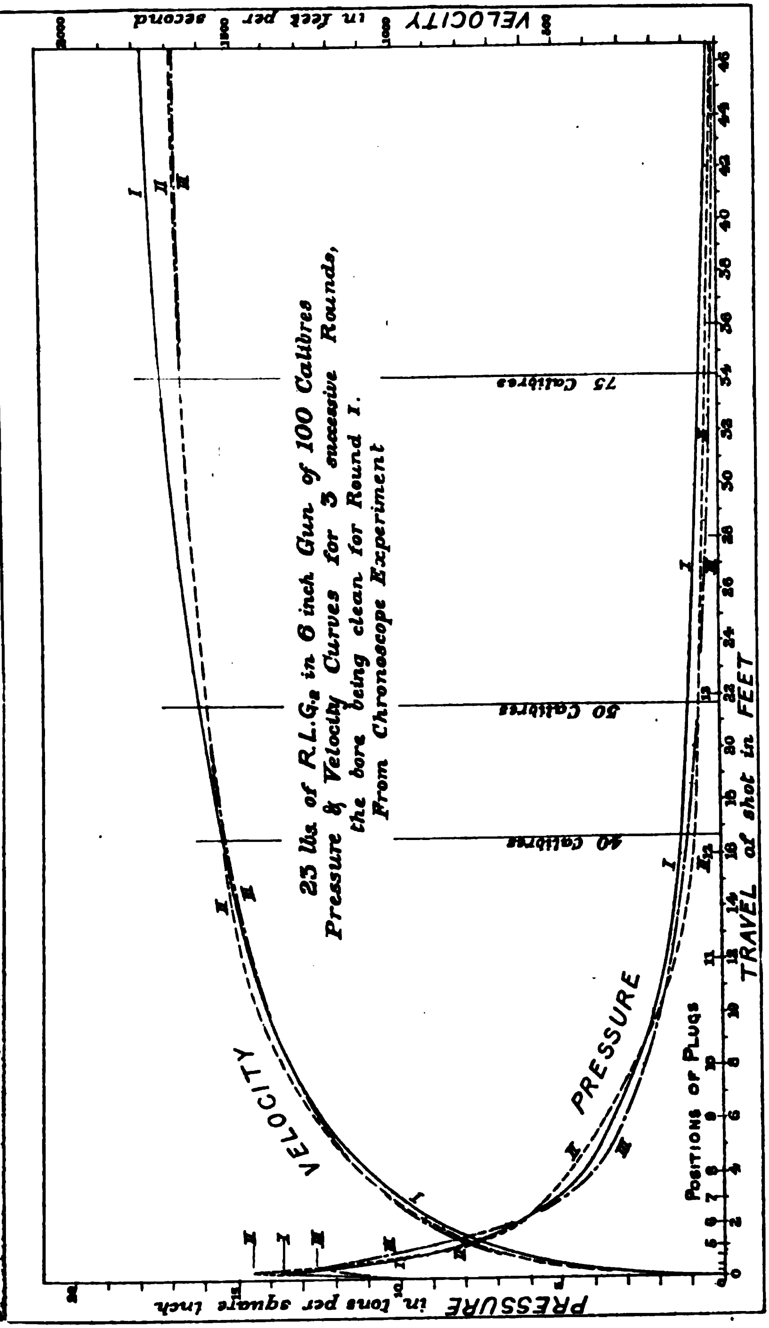

25 lbs. of R.L.G.₂ in 6 inch Gun of 100 Calibres
Pressure & Velocity Curves for 3 successive Rounds,
the bore being clean for Round I.
From Chronoscope Experiment
VELOCITY in feet per second
PRESSURE in tons per square inch
TRAVEL of shot in FEET
POSITIONS OF PLUGS
75 Calibres
50 Calibres
40 Calibres
VELOCITY
PRESSURE

Fig. 3.

Table showing the Velocities and Energies realised in a 6″ Gun with the undermentioned Explosives.

Nature of explosive and weight of charge.	Length of bore, 40 calibres.		Length of bore, 50 calibres.		Length of bore, 75 calibres.		Length of bore, 100 calibres.	
	Velocity.	Energy.	Velocity.	Energy.	Velocity.	Energy.	Velocity.	Energy.
Cordite, 0·4″ dia., 27·5 lbs.	2794	5413	2940	5934	3166	6950	3284	7478
Cordite, 0·35″ dia., 22 lbs.	2444	4142	2583	4626	2798	5429	2915	5892
Cordite, 0·3″ dia., 20 lbs.	2495	4316	2632	4804	2821	5518	2914	5888
Ballistite, 0·3″ cubes, 20 lbs.	2416	4047	2537	4463	2713	5104	2806	5460
French B.N., 25 lbs.	2422	4068	2530	4438	2700	5055	2786	5382
Amide Prismatic, 32 lbs.	2225	3433	2331	3768	2486	4285	2566	4566
R.L.G.₂, 23 lbs.	1533	1630	1592	1757	1668	1929	1705	2016

Figs. 1 and 2 are summarised in the annexed table, which shows for each nature of explosive the advantage in velocity and energy to be gained by correspondingly lengthening the gun.

Fig. 3 is an interesting illustration of a point to which I have elsewhere adverted. Cordite and ballistite leave no deposit in the bore. Round 1 with R.L.G. was fired with a clean bore. The difference in velocity between round 1 with a clean bore and rounds 2 and 3 with powder deposit in the chase is very clearly marked, and it will be noted that in this instance the effect of the foul bore is only distinctly shown when the length exceeds 40 calibres.

From 40 calibres onwards the loss of velocity due to a bore encrusted with deposit is very distinctly shown.

II. "Measurement of Colour produced by Contrast." By Captain W. de W. Abney. C.B., D.C.L., F.R.S. Received June 5, 1894.

No definite measurements, as far as I am aware, have been made of the change in colour produced by contrast, except in a small work of my own in which results were given in terms of colour mixtures, and earlier by a brief reference in a work by Rood, in which the change produced was endeavoured to be matched by means of rotating disks.

The method of registering any colour in terms of some definite wave-length of light, together with white light (see 'Proceedings Royal Society,' vol. 49) renders the registration of any colour readily effected, and by applying it to the contrast colours, very fair results have been obtained, which cannot be very far from the truth. It is usually stated that the contrast colour produced on a white surface by an adjacent colour is the complementary colour, of course largely diluted with white light. I should like to point out that in the first place we have to know what a complementary colour is, and in the second what the added white light may be. As a matter of fact the kind of white light employed has to be defined before it can be stated what the complementary to any colour may be. If, for instance, we wish to define what the complementary of orange may be, we must know what is the nature of the white light before we can give the complementary. Suppose we take the white of daylight, or of the electric light, we know that to make a white of this character we must add a certain quantity of blue of a certain wave length to the orange. When it is produced under these circumstances, the blue is the complementary to the orange. Suppose, however, we wish to know the complementary to the orange, in what is called the white light of the amyl acetate lamp, or of a candle, we are at once met by a difficulty.

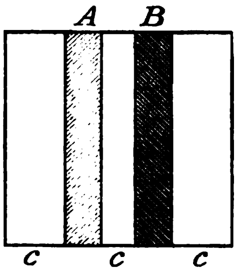
The colour of the light of these two sources, can, as far as the eye can distinguish, be very closely matched—if not exactly—by an orange ray in the spectrum. Evidently then in such a case there can be no complementary to this ray. We know as a matter of observation that these lights do contain a certain quantity of rays of the higher refrangibility, but so small proportionally to that found in daylight that it is negligible. Again, we may take a light, such as the oxy-hydrogen light, and may match its whiteness by placing in the electric light spectrum three slits, one in the red, another in the green, and another in the blue, mixing the three rays, and altering the apertures of the slits as required. The complementary of the red for the oxyhydrogen white light will be obtained by shutting off the red ray, leaving the mixture of the green and blue rays. Keeping the slits in the same position in the spectrum, a match may next be made with the white light of the electric (arc) light. The complementary of the red may again be found as before, when it will be found that the mixture of green and blue forming it will be bluer than in the case of the oxyhydrogen light.

From the above it will be seen that no complementary colours can be definitely stated unless the quality of the white light be known. Sunlight and daylight being always yellower, at sea level, the lower the altitude of the sun, it follows that any attempt to fix accurately the wave-length of the complementary for daylight to any ray of the spectrum must be exceedingly difficult. Wave-lengths of complementaries are given, however, in various text-books, but without any statement as to the quality of the white light to which they refer. It follows that if we do know the complementary colour, then the white light which has to be added to it in order to match the contrast colour must refer to the same white to which the complementary is referred.

In the experiments which were undertaken as to the true colour produced by contrast the light employed was that which I have always used in colour experiments, viz., that emitted by the crater on the positive pole of the electric light—a light which is unchangeable, and which can be relied upon as always being of the same quality, the relative luminosities of the different parts of its spectrum being fixed. Two complete sets of apparatus for producing colour patches as described in the Addendum in "Colour Photometry," ('Phil. Trans.,' 1886) were provided each with its electric light. Each colour patch was thrown on the whitened surface of a cube (No. II) of $1\frac{1}{2}$ inch side placed 12 inches apart from one another. With No. II instrument the colour contrast was formed between white and a diluted spectrum colour, the colour emerging through a slit placed in the spectrum and forming a patch on the cube, and the white being that reflected from the first surface of the prism, and re-reflected by a silvered mirror, a magnified image being thrown, by means of a

lens, also on the cube. A thin rod $\frac{3}{8}$ inch diameter placed in the paths of the two beams caused two shadows to be cast on the cube, one illuminated by pure white light and the other by the spectrum colour. These were separated from one another by an interval illuminated by a mixture of the spectral colour and white light, and on each side of the shadows the same diluted colour was to be found. The appearance of the side of the cube was as below.

FIG. 1.



A was a stripe of white light, B of colour, *c c c* of the same colour diluted with white. The intensity of the D sodium light thrown on the surface was 0·5 of an amyl acetate lamp at 1 foot, the intensity of the other colours can be obtained from the luminosity curve in the 'Phil. Trans.,' "Colour Photometry," Part III, 1892.

The patch of colour from instrument No. I was thrown on the face of a cube (No. II) 1 foot away from the first cube, and was used to match the contrast colour produced on A, fig 1. The beam of white light also fell on the same face of the cube, and the intensities of each could be altered at will, that of the colour by opening or closing the slit through which the colour came, and that of the white light by rotating sectors. By this means any dilution of colour could be secured. It may be mentioned that the effect of using a strip of the face of the cube equal in width to the width of A was tried, but no advantage was found by so doing. The method of procedure was as follows:

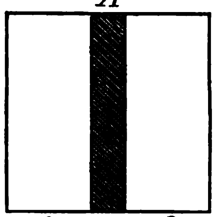
With instrument No. II the colour to be used and the white beam were thrown on the face of the cube No. II. The luminosities of the two were made as nearly equal as possible. With instrument No. I a colour, which it was judged was nearly the dominant colour of the contrast colour on A, was thrown on the face of the cube No. I and white light added. When it was found that a match was perfected by slight changes in the colour and in the intensity of the added white, the scale No. of the colour was read, from which the wave-length would be determined, and the relative luminosities of the white and the colour measured directly. The aperture of the slit

used in making the match being known the dilution of the colour would be determined readily.

It was found that a slight change in the contrast took place after repeatedly shifting the eyes from the one cube to the other. For instance, the contrast caused by green appeared to lose a little of its red hue, degenerating into a brown-yellow. To avoid this, an artifice was employed, which appears to be completely successful. An ordinary box stereoscope, with the lenses removed, was mounted on a stand, and in such a position that when the left eye only saw cube No. II, the right eye saw but No. I cube. Thus, the right eye never saw the contrast colour, whilst the left never saw the match. In this way, by alternately changing the direction of the eyes to the two cubes, a match could be readily made. When the match was considered satisfactory, the eyes were directed to a moderately weak white light, and, after a short interval of time, turned to the two cubes, when, if the contrast colour on the one cube and the mixed colours on the other appeared to match accurately, the necessary readings were taken.

FIG. 2.

A



C C

Subsequently it was found more convenient to move the rod placed in the paths of the two beams of the instrument No. II, so that only one shadow appeared, as in fig. 2. In fig. 2 the stripe of white light, A, is shown. It is obvious that the stripe of colour could be equally well isolated. There is no difference in the contrast colours created in the white by this plan, so that only one table of results need be given.

It will be seen from the table that different and representative parts of the spectrum were used, being the red, yellow, green, blue, and violet, and that in every case the contrast colours provoked in the white could be matched by a single colour of definite wavelength when diluted by white light. If the contrast colour caused by the green were its complementary diluted by white light, it should be by a purple, which requires a mixture of red and blue, whereas it is an orange. The fact as to whether the contrast colour

Table I.—Diluted Background.

Colour contrasted with white.		Colours produced by contrast.	
Wave-length of colour.	Luminosity in terms of amyl acetate lamp.	Dominant wave-length of contrast colour.	Proportion of white to colour. White = 1.
672	0·15	483	0·054
636	0·22	484	0·057
612	0·44	485	0·066
598	0·46	487	0·070
585	0·50	489	0·100
569	0·49	671	0·120
558	0·44	610	0·165
541	0·33	598	0·165
517	0·13	592	0·170
499	0·07	587	0·175
481	0·023	585	0·200
466	0·012	583	0·250
All violet.		581	0·300

as matched could ever make white when mixed with the colour which caused it was very readily proved. The two colours were thrown on the same cube, and the proportions of the colours altered. In some few cases there was a very close approximation to the formation of a white which matched the electric light, but in the majority no match could be made.

Another set of experiments further exemplified this. In instrument No. II three colours were chosen—one in the red, another in the green, and the third in the violet. The same three colours were found in instrument No. I, and three adjustable ones placed in them. With these three slits a match was made with the white of the electric light in the first instance—a contrast between white and the red was then formed on the cube, illuminated by No. II instrument. The red was then shut off from instrument No. I, and the mixed violet and green lights were diluted with white light, but in no state of dilution did the white stripe as coloured by contrast appear of the same tint as the complementary colour of the red as obtained from the diluted mixture. The same negative results were obtained by making the contrast with the green. With the violet a much nearer approach was made.

This experiment was varied by matching the light from an Argand gas burner, and by forming the contrasts by means of the same quality of light. The same negative results were again obtained.

The difference, if any, was next observed between a contrast made by a saturated colour and that given by the diluted colour.

In order to get a stripe of white inclosed between two saturated stripes of colour a Vernon Harcourt screen was employed instead of a rod. The principle of this may not be known generally, so a brief description of it may be necessary. It consists of a rectangular metallic sheet of about two inches wide, in which two broad slits are cut and separated from each other by the width of the slits. This sheet, if placed in the path of the beam allows two stripes of colour and of white to pass. By carefully adjusting the position of this screen a stripe of white may be enclosed between two stripes of colour. The results are given in Table II.

Table II.—Saturated Background.

Colour contrasted with white.		Colour produced by contrast.	
Wave-length of colour.	Luminosity in terms of amyl acetate lamp.	Dominant wave-length of contrast colour.	Proportion of white to colour. White = 1.
672	0·15	481	0·015
636	0·22	485	0·020
612	0·44	486	0·022
598	0·46	487	0·024
585	0·50	491	0·025
569	0·49	671	0·035
558	0·44	611	0·052
541	0·33	598	0·066
517	0·13	590	0·066
499	0·07	585	0·068
481	0·023	583	0·068
466	0·012	582	0·070
All violet.		580	0·070

The contrasts with gas light, using the same light to dilute a spectrum colour in instrument No. 1, were also measured, and these are given in Table III.

Table III.—Contrasts in Gaslight.

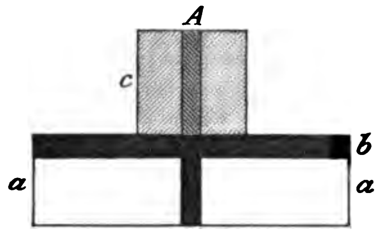
Wave-length of colour.	Dominant wave-length of contrast.
636	485
585	590
558	598
499	592
465	589
All violet.	588

There are such small differences in the wave-lengths of the contrasts produced by the diluted and saturated colours that it may be presumed they are due to error of observation, although each table is derived from the mean of several observations extending over a period of three years. It may be interesting to state that in every case the extremes in the one series embraced the mean value tabulated in the other series, and that in no case did the mean differ from any single observation more than λ 2·5.

There is, however, a very simple means of noting the accordance between the contrasts caused by the diluted and saturated colours. With one instrument the contrast caused by the saturated colour was shown on one surface, and with the other that by the same colour, but diluted, on another surface, so that the two could be directly compared. To the eye the only difference between the two was in the amount of dilution of the colour produced by contrast; otherwise they appeared absolutely identical.

An endeavour was made to ascertain at the same time what dark interval between the white and the colours would prevent the contrast being appreciable. To do this a cube with a whitened surface was placed as shown on the top of another white surface with a black interval between the two (fig. 3).

FIG. 3.



The colour patch was thrown so as to fall only on the cube *c*, whilst the white beam illuminated the white surface *a* as well. When the white beam was also thrown on another cube a foot away it was practicable to form an idea of the colour of "*a*." The effect was curious and interesting. When the black band *b* was just ¼ inch in depth, whilst the white stripe A appeared strongly coloured, *a* appeared *very nearly* white, and if by an artifice saturated colour surrounded A it was pure white. If black intervals were placed on each side of A the colour in A did not disappear, but appeared to be more diluted, probably owing to contrast in the white caused by black, but the colour still remained. If, however, a black interval was on one side of A (that is by placing the shadow against the edge

of the square and making the black interval between the colour and A), when the colour was saturated white appeared perfectly white, whilst if dilute just a shade of contrast colour was visible.

By placing a diluted coloured space in contact with a pure white space which was in its turn in contact with a saturated colour, it became possible with several colours to make the diluted colour appear white in contrast to the contrast colour itself. With red this became impracticable, and for a reason which will be apparent in a paper which I propose shortly to communicate.

In this paper it is not intended to include the changes made in undiluted colours by contrast with white or other undiluted colours, or between colour mixtures. It may, however, be said that except for the red and the violet there is a tendency for the two colours to become more widely separated in the spectrum. Thus with red and yellow the red remains of the same hue, but appears slightly more saturated, whilst the yellow appears greener. This would necessarily follow from the contrast colours produced on a white stripe by saturated colours.

The reasoning given to explain contrast colours on the Young theory, or on that of Hering seems insufficient, and very hypothetical. I would, however, call attention to a curious phenomenon which General Festing and myself described incidentally in "Colour Photometry, Part III." When getting the final extraction of colour from a red ray by a direct comparison with very faint white it was found that when apparently both appeared of the same grey hue, if the white light were increased in intensity, the colour of the red immediately appeared, and it was only after making a comparison for colour with the stronger white that the red colour truly appeared to be colourless. In this then evidently the part of the retina on which the red colour was received was stimulated by the white adjacent to it and to such an extent that the supposed extinguished colour reappeared. Presumably then whilst the retina is excited by diluted colour, the part on which the pure white may fall may also be excited by it, and not necessarily by the exact complementary colour, since the eye is more sensitive to some colours than to others. Whatever view may be taken of this hypothesis, must not, however, be allowed to detract from the results of the experiments, which are facts—recorded and observed with all possible care—and after due precautions were taken to avoid error.

III. "On some Phenomena in Vacuum-tubes." By Sir DAVID SALOMONS, Bart., M.A., V.-P., Inst. Elec. Engrs. Communicated by Prof. D. E. HUGHES, F.R.S. Received April 30, 1894.

This paper is a contribution upon the phenomenon known as striæ, or bands, in vacuum tubes.

As far as I can learn from the sources of information available to me, no one has yet discovered how to produce a predetermined number of bright and dark bands in a tube having an open or free path.

After a prolonged investigation I have succeeded in producing this result.

This first step having been attained, it is evident that a number of experiments are available for confirming the theories at present held in regard to the subject, or possibly for modifying existing views, or even to form some additional theory, if necessary.

I do not think that it would be judicious, at the present stage of my experiments, which are by no means complete, to enter upon any theoretical considerations, although some conclusions might suggest themselves which, however, until the work has been greatly extended, would possibly lead to error.

It would appear that the main efforts of those experimenting with vacuum-tubes have been in the direction of securing very high exhaustions and using currents of very high electromotive force. In many cases also currents having a high frequency have been employed.

When I began these investigations, about twenty years ago, the chief difficulty was to know at what point to commence. After due consideration I decided upon the following course:—

A very large number of vacuum-tubes were "lit up," and all tubes which showed a somewhat similar phenomenon were carefully examined, and their characteristics noted. It is probable that the number of tubes so examined considerably exceeded a thousand, perhaps several thousands.

At last it became quite clear that, to produce a definite phenomenon, the tube must be given some definite characteristic; and having settled this point I was enabled to start upon a systematic investigation.

There is no object to be gained by detailing the reasons which led me to work in the manner I did, and, therefore, I will be content to give the results.

When I use the word vacuum-tube I employ it in the ordinary sense. In all the experiments to be described the tubes contain ex-

hausted air, and the current employed is an alternate current. The experiments here mentioned must, therefore, be regarded as a first instalment. They will have to be repeated, with an intermittent direct current and with tubes containing various gases, and probably also tubes containing various vapours, and all these at various exhaustions and temperatures.

I have already made a large number of experiments employing tubes containing different gases and with direct, as well as alternate current. I may, therefore, mention that the phenomena about to be described, when using the alternate current, appear to be the same with the direct current, except that the phenomena peculiar to direct currents, in the form of the bands, make themselves apparent. But, until these experiments have been completed, I will not refer to them any further in this paper. I find it necessary to work in the contrary manner to that which is usually adopted.

1. The alternations are made so slow that blinks are produced in the tube under observation, which correspond to the reversals; and then the alternations are increased in speed until the tubes appear continuously lightened. This is my starting point. I have the means of raising the frequency when desired. The apparatus for producing the alternate current, in the first instance, is a Pyke and Harris alternator driven by an electromotor, hence as the frequency increases so does the electromotive force; I am therefore obliged, when describing the experiments, to employ the expression "electric energy" or "current" in order to avoid confusion.

2. Since I use currents of such low frequency the electromotive force also is very low; and the quantity of current traversing any tube is small.

3. The tubes are not very highly exhausted, they are only exhausted to approximately 0·5 mm. of mercury, according to the usual mode of comparative measurement. But I find that, whether the tubes are highly exhausted or not, provided the current passes, all the phenomena are the same. At a very early stage of the investigation I observed that the ordinary methods of working, *i.e.*, with currents of high electromotive force, masked the effects I was seeking.

In giving the subject a logical sequence so that each step may be noted, I have no doubt that I shall describe several effects which are already known. But to omit these, which I am unable to single out, would be to lose the thread of the history.

In a scientific paper it is usual to prove some definite point or to show some special new phenomenon. It is therefore desirable to point out the object aimed at in this communication.

The object I originally had in view was simply to discover a method by which vacuum-tubes could be made to give a prede-

termined number of bright and dark bands, as it appeared to me this would be the first step in examining the laws which govern their production. In this I succeeded at a very early stage. But, in order to make the proofs more convincing, other experiments were entered upon which brought to light a number of new points of considerable interest; and many of these experiments appeared to throw considerable light as to the origin of the bands.

The object of this paper, therefore, is, first, to show the methods by which a definite number of bright and dark bands can be produced in a vacuum-tube; and, secondly, to describe a number of interesting phenomena which have a bearing on the production of the bands in general.

The first step is to describe the apparatus employed in making the experiments. A small direct-current motor is coupled directly to the smallest sized Pyke and Harris laboratory alternator. The alternate current produced can be made to vary its E.M.F. according to the speed given to the motor and also by varying the exciting current. The E.M.F. of the alternate current can be made variable from 0 to 100 volts, and the maximum current which the machine is intended to give is 3 ampères. The pressure of this current is raised by means of a Pyke and Harris oil transformer or with a Salomons and Pyke combination transformer. Sometimes one form of transformer is used and sometimes the other. It is usual to give the exact electromotive forces of the current employed, as well as its periodicity, but in the following experiments it is not necessary that this should be known accurately, from the very nature of the experiments, because, as already mentioned, the experiments were started with exceedingly slow alternations and a very low E.M.F., which were gradually raised, the phenomena being watched throughout as they varied.

It is essential that the speed of the motor should be under complete command, and that the speed should be made variable during any experiment. To attain this end I employ a Kelvin rheostat and a Wirt rheostat.

All the tubes employed, so far as their exterior form is considered, may be regarded as practically of one type; long tubes of varying lengths and large diameters, many of them containing small devices which consist of little glass disks, glass rods, and other arrangements for modifying the nature of the electric discharge, and, for want of a better name, I term them "deflectors." Although, perhaps, the word "moderator" is more applicable, it has another meaning, and its employment here might give rise to confusion.

In many of the experiments to be described the bands appear to be repelled from some part or parts in the tube towards the electrodes. I feel some difficulty in selecting language suitable for de-

scribing this effect, for although to the eye repulsion is evident, it may not, from a scientific point of view, be correct in fact. The appearance may be explained in a variety of ways, but in order to simplify description of the experiments I treat the appearance as a repulsion. However, it must not in consequence be inferred that repulsion really does take place.

The experiments are here given in their logical order, although in point of fact they were not originally made in this sequence. In all investigations it occurs almost invariably that an experiment is made which suggests the previous steps that are necessary to obtain a logical order, and indeed some of these steps are frequently unobtainable. Therefore, it is very probable that what I believe at present to be the sequence may yet have missing links.

Some of the conclusions which may be drawn from the following experiments are:—

That bands may be produced with greater facility in small tubes than in large, and that they become more accentuated probably on account of the inequality of the diameter of such tubes.

That for the production of bands, the glass of the tube itself appears to play a part, since the bands are difficult to produce unless they reach to the glass of the tube.

That an exceedingly minute current produces bands which to the eye, in most instances, disappear when the current is somewhat increased, and on further increasing the current they become visible again. I believe that in all previous investigations it has been stated that the bands cannot be produced until a considerable current is passed. I refer to investigations by Messrs. Warren de la Rue, Gassiot, and others. My experiments prove the contrary. The probable reason why these statements were made is due to the fact that with the apparatus employed at that time such small currents could not be easily produced. When the minute current is increased, and the bands seem to disappear, I think this is only an optical illusion; the bands are there, but too faint to be seen, perhaps in consequence of the dark bands being so narrow that they escape observation.

That, when an electric discharge takes place in a large tube in which is placed a partition pierced with a hole, "a forcing effect" frequently appears to be produced. Any bright bands being produced at the hole in the partition may give the appearance of being pushed through to the side of the tube which has the greater length. This phenomenon is mentioned because it is apt to mask many effects, unless the current is suitably adjusted.

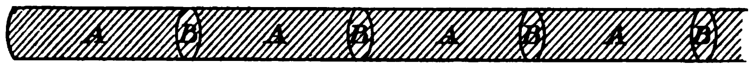
That it is not impossible, after the first trace of light becomes visible in a tube when passing a very minute current, that the dark bands subsequent to this stage are illusory, and that they are really

the bright bands; and what appear to be the bright bands consist of overlaps which produce double the brightness of the so-called dark bands. In reality, therefore, the bright bands indicate the position of the dark bands. (See fig. A.)

Case 1.

A = Bright bands. B = Dark bands.

Case 2.



Bright bands A expanded overlap at dark spaces B, which now appear twice as bright as at A, and the spaces A appear dark by contrast.

FIG. A.

That by devices bands can be produced in a large tube occupying only a small portion of the cross sectional area, at any rate so far as the eye can discern.

That, when employing Professor Crookes' tubes for illustrating experiments on radiant matter, if suitable conditions are observed, striæ are formed in these tubes.

That, in tubes having exceedingly small electrodes, and apparently not capable of producing striæ, these can be shown to exist if very minute currents are employed.

That the tube, when made to act as a condenser, permits more current to pass.

That from the above considerations it is not unlikely that a view, which has been held, in regard to the probable origin of the bands, that they consist of a series of discharges through the tube, is true; that the nature of such discharge can be varied by suitable devices placed within the tubes, and that the examination of the nature of the discharge can be best made with very minute currents, that is to say, currents so small that, if made any less, the tube would no longer show any sign of light.

(N.B.—The number against each figure corresponds with the number of the experiment. This accounts for the absence of fig. 10. The experiments were made with the tubes supported horizontally (with the exception of tube fig. 12). This position was chosen for the sake of convenience, the results being the same for all positions. Tube fig. 14 must be placed horizontally in order to shift the movable disk).

Experiment 1.

A plain tube of large diameter with aluminium brush electrodes at the ends is employed. (See fig. 1.) On passing an electric discharge, with very slow alternations, at first the tube appears dark, and the speed of the alternator then being slightly increased, the light just becomes visible within the tube, and there is seen a few bright bands with dark spaces intervening. These bands are convex, the convexity being towards the centre of the tube in all cases. The bands do not extend to the centre of the tube, consequently the convex sides of the bands at each end of the tube face one another, and do not meet in the centre of the tube. The motor is then stopped, and the falling light in the tube is carefully watched. It can then be noticed that the moment before the current dies out, and the bands disappear, one or two more appear nearer to the centre. On the other hand, if the current is increased the bands are driven towards the electrodes until they disappear altogether. The tube is then simply lighted throughout.

The experiment is now repeated when a current is adjusted to produce the bands, and the centre of the tube is placed to earth by resting the hand upon it or in any other convenient manner, when it

Fig. 1.

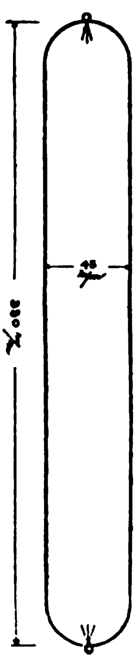
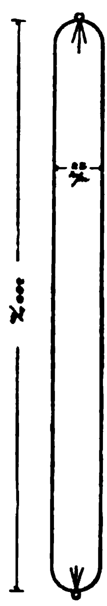


Fig. 1a.



will be observed that the bands approach towards the centre, but still do not fill the tube; sometimes one or two new bands appear. It is difficult at present to say why this should be the case, for, apart from other views which may be taken, the following two conditions might exist:—

1. If the apparent repulsion of the bands is due to the electrification of the glass of the tube, then an effect is produced somewhat similar to that in the gold leaf electroscope, and by placing the hand upon the tube, a discharge being produced, the bands will approach in the same way as in the case of the leaves of an electroscope when discharged, but the glass being a bad conductor, the discharge is partial only.

2. The view might be taken that when the hand is placed on the tube a condenser is formed, and the surface of the glass within the tube becomes more highly electrified, and the approach of the bands is due to this cause. If this explanation is true, the ocular effect of repulsion is not really due to repulsion.

The condenser action appears to be confirmed by my experiments, and is very prettily shown in the following manner:—

Adjust the current in any vacuum tube until the discharge is just visible. Now carefully reduce the current until the tube is dark, *i.e.*, when there is no visible discharge. If the hand is now placed on the tube the latter will light up brightly. If the adjustment is made with sufficient care, the very fact of placing the hand within an inch or so of the tube will cause it to light up as if by magic. The repulsion effect may also be true, although it appears to me evident that more current flowing into the tube, when the outside is earthed, is in no way caused by any discharge from the interior of the tube.

A tube is now taken of much smaller diameter, 25 mm., the bands will now be formed the whole length of the tube; other phenomena are the same as with the large tube. (See fig. 1A.)

Experiment 2.

A similar tube was employed, but it contained in the centre of its length a very slight glass rod, attached to the side, supporting a short thick glass rod lying along the axis of the tube (see fig. 2, where dimensions are given, as in the case of other figs.). The object of this experiment is to see whether the repulsive effect would be increased; or, if not increased, whether the phenomenon could be made more clear. The latter proved to be the case. In passing the current in the same manner as in the previous experiment, bands were formed throughout the length of the tube, one appearing at the centre of the little rod and one at each end or near the ends of the rod. On in-

Fig. 2.

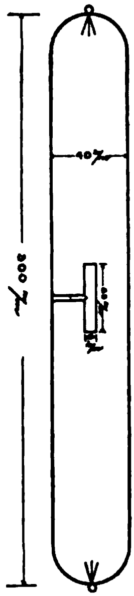
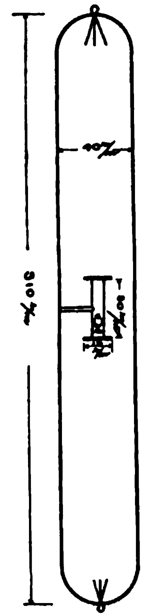


Fig. 2a.



creasing the current the bands are driven towards the ends of the tube. A point is reached when the rod is clear of bands, only one appearing at each end of the rod. Still further increasing the current the bands are farther driven back until at last the tube appears filled with light without any trace of striæ. From these two experiments I conclude that, in a large number of cases, the phenomenon of bands is masked in consequence of too much current being employed. Similar tubes, with glass discs at the end of the rod were used, and with same effect, see fig. 2a. Some of the experiments which follow clearly explain how this masking effect is produced.

Experiment 3.

It is well known that a bright band is formed at any little projection placed within a vacuum-tube. For instance, if a rod of glass is placed within such a tube along its axis, and has upon it little beads of glass or any other material, a bright band will be produced at these places. It appeared to me that, from Experiment 2, it was not improbable when considering the "repulsion effect" that these bright bands really consist of a pair in close contact.

In order to examine this question more closely I use a tube (fig. 3) which contains two thin glass disks placed upon a glass rod at one end of the tube. On passing the current, sufficient to light up the

tube, a distinct bright band is seen on each side of each glass disk. Several tubes are used in this experiment, and in many instances by lowering the current the time arrives when the two bright bands, on either side of a disk, appear as one. When the current is increased these bands leave the edges of the disks to some distance from them, as if they are repelled one from the other in consequence of more current flowing through the tubes and creating a greater electrification, if it be due to this cause at all.

It is further seen that throughout the free portion of tubes con-

FIG. 3.

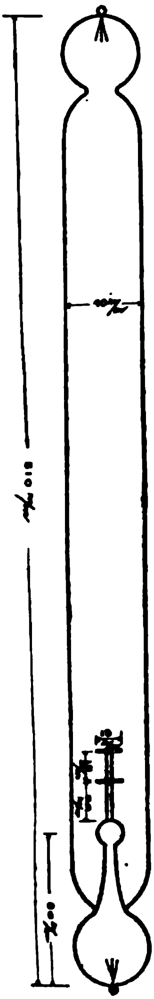


FIG. 3A.

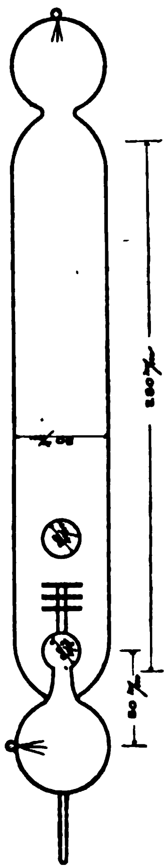
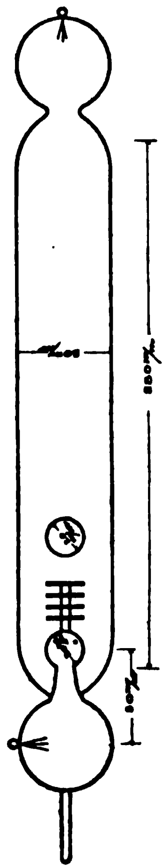


FIG. 3B.



structed in this manner, the bright bands are repeated along them, the distance corresponding with the distance between the bright bands formed at each side of any disk. On increasing the current, very considerably in consequence of the bands on the glass disks receding from the latter, a confusion is set up throughout the tube, *i.e.*, the bands all mix together, especially as they become somewhat wider at the same time as they recede from the glass disks, till finally the whole tube appears generally lit without any bands being seen. Some tubes will melt before obtaining this effect.

I, therefore, conclude that the absence of bands is generally due to this cause, *i.e.*, the bands are really there, but in consequence of too much current they have become expanded and overlap one another so often as to render their existence invisible. Some very instructive experiments for the examination of this effect will shortly be described.

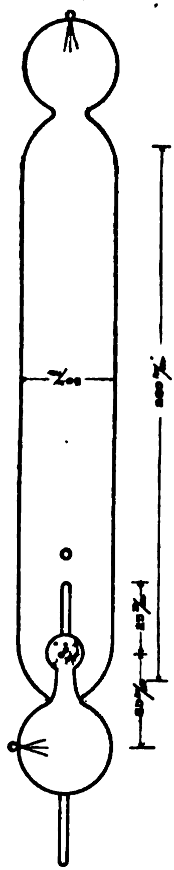
Tubes with three and four disks on the rods were also used, and these produced similar effects. (See figs. 3A and 3B.)

Experiment 4.

This experiment and some of those which follow are made with a view of determining the possibility of producing a given number of bands in a vacuum tube. The possibility of doing this might already be inferred from the experiments described. If it is generally true that any two bands produced at one end of a tube are repeated throughout the tube at the same distance apart as the first pair formed, and if it is true that any impediment within the tube will produce a pair of bands, then the problem is solved; and I think the following tubes show that such is the case.

The tube shown in fig. 4 has its ends contracted and re-expanded into bulbs, in which the electrodes are placed. At one end, where the contraction exists, a short tube is melted on, which carries a little hollow sphere of glass with three holes so situated that, if a line be drawn from the centre of any hole to the centre of the glass sphere, an angle of approximately 45° is made with the axis of the tube. There is melted on the top of the glass sphere a short rod projecting into the free part of the tube, the axis of the rod coinciding with the axis of the tube. When the current is passed there *should* be formed a bright band at the base of the rod close to where the current issues from the three holes in the sphere, and another one at the free end of the rod. Then throughout the tube there *should* be produced bright bands equidistantly placed, such distance being equal, or approximately equal, to the length of the little rod; and, on passing the current, this proves to be the case. When the amount of current passed is exceedingly small, distinct, narrow, bright bands become

FIG. 4.



visible, with absolutely dark, wide spaces between them. On increasing the current, the bright bands expand, the dark spaces growing narrower till at last the bright bands touch one another and then overlap. Then the paradox occurs that the dark bands appear bright and the bright bands appear dark; *i.e.*, the bright bands overlap where the dark spaces should be, and therefore appear doubly as bright as the remaining portion of the bright bands, which now appear dark by contrast.

From experiments conducted on other tubes I think it is quite clear that this is the usual condition of things, and that it is exceedingly rare that the true black bands are really seen. But in this experiment there is one point of interest which must not be overlooked. It is, that whether the current be small or large, the rule set up by the little rod at the end of the tube cannot be upset. The

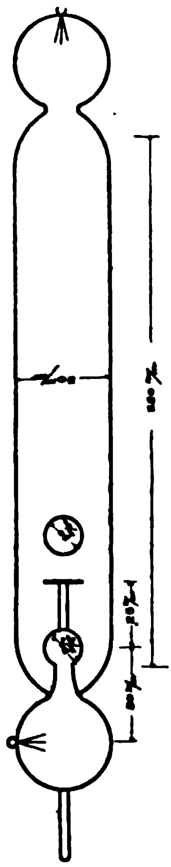
number of bands in the tube will not advance nor recede, nor alter their number in any way; neither can they be made to disappear by increasing the current as in other cases. The only thing that happens is that the bright bands grow wider as the current is increased till the paradox referred to is apparent.

This same experiment is made on a number of tubes built in a similar fashion, but having different dimensions. The result, however, is always the same. The overlapping of the bright bands may occur again and again, so that the dark spaces are first bright, then dark, then again bright, and so on, until the tube is destroyed by the excess of current passed.

Experiment 5.

I find that if a tube is employed similar to that in the last case, but with a glass disk placed at the end of it (see fig. 5), the same

FIG. 5.



result follows. The object of this experiment was to see whether the glass disk would set up its own bands independently of those due to the rod, but the rod appears to gain the mastery. Probably the two bands formed by the glass disk are driven (or repelled) into one by the rod.

Experiment 6.

I next tried the effect of contractions to see whether the same result would not be produced as if the deflector rod were employed. Several forms of tubes are used: in some cases a little glass sphere, as described in the previous experiment, without the rod attached to it; in other cases the tube was contracted for a definite length and then, expanded; in other cases disks of glass and of mica were inserted with holes in the centre, as shown in figs. 6, 6A, and 6B. The same results are produced as with the rod; the distance of the bright bands apart being equal, in the first tube, to the distance between the holes in the glass sphere and its electrode; in the second tube, to the length of the contracted tube; and in the third tube, to the distance between the holes in the disk and its nearest electrode.

When a tube, containing a glass or mica screen with a hole in it, is employed, one of the three effects may be produced:—

1. Broad bands throughout the tube, as already mentioned.

2. Very narrow bands, their distance apart being equal to the distance between the two bright bands formed on both sides of the screen in the tube.

3. No bands in the tube, or only a confusion of bands, and no distinct bands on either side of the disk. Unless this circumstance were pointed out, it might prove misleading to anyone trying the experiment. I tried a large number of tubes built up in this manner, in order to discover the cause of the different phenomena.

It would appear that broad bands are produced when the distance between the disk and its nearest electrode is suitably adjusted. If the screen is placed far away from the electrode, bands are produced on each side of the disk, and they are reproduced throughout the tube. If the screen is placed nearer to its electrode, the two disks appear to be driven into one, and broad bands are produced. If the screen is placed still nearer to the electrode, then what should be the two bands on either side of the disk are driven through the hole, and appear as a hemisphere of light on the side of the tube which is the longest. In this case bands may or may not be produced, and, if present, they are irregular.

These three effects can be produced by varying the current in many instances, and very probably in all cases; but, as a rule, the tubes break down before all the effects can be shown.

Again, plain tubes are employed with electrodes of varying lengths.

Fig. 6.

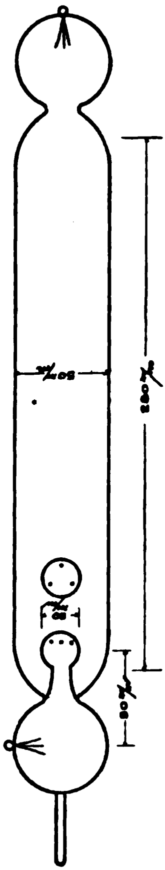


Fig. 6a.

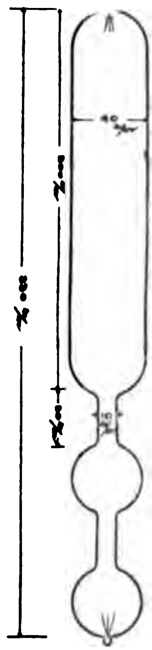
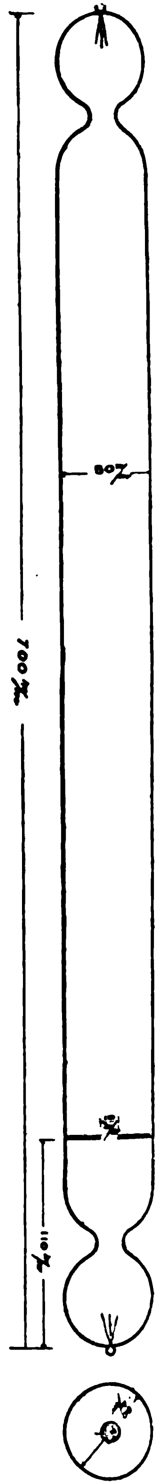


Fig. 6b.



The length of the electrode acted in the same way as did the deflector rod in Experiment 4. It is, therefore, clear that in arranging Experiment 4 it was necessary to have the length of the rod some multiple of the distance between the holes in the glass sphere and its electrode; also that the length of the glass rod should be some definite multiple of the distance between the contraction of the tube at the farther end and its electrode; also that the length of the electrodes should be short compared with any of these distances; otherwise a confusion of bands would be set up.

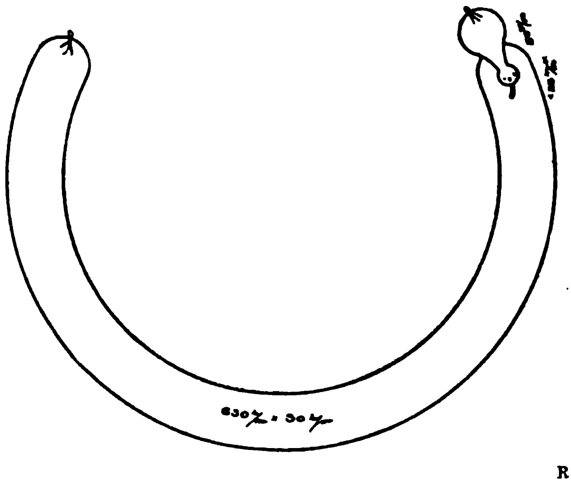
The experiment with the disk of glass placed at the end of the deflector rod showed that any effect which could be produced by this disk alone was overpowered by the effects produced by the rod. This would appear evident, since the two bands which would be formed by the disk would practically be driven (or repelled) into one by the action of the rod; the independent action of the electrodes in the case of Experiment 4 being absent may be due to the same reason.

I had considerable difficulty in getting over these points, for, at first, confused results were presented in some tubes and not in others, and it was only after investigating the matter, as here mentioned, that I was able to construct tubes without difficulty to give definite results when these various details were attended to.

Experiment 7.

Various forms of curved tubes are tried, some of them bent almost into a circle. In several instances the little hollow glass spheres with short rods bent to the curve of the tube are inserted, and in other

FIG. 7.



cases screens across the tubes with holes in them, and other tubes similar to those already described, the object being to see whether the effects produced are the same as if the tubes had been straight, and this proved to be the case. One tube is shown in fig. 7.

Experiment 8.

This one consists of a number of tubes containing no devices of any kind. But in one case there are two electrodes at one end, and one electrode at the other end of the tube. In another case a tube has two electrodes at each end. Another tube has very small electrodes, while others have various sizes of electrodes. In all these instances bands are produced, but the smaller the electrodes the less current has to be passed to show the bands, and the closer they appear together. (See figs. 8 and 8A.)

FIG. 8.

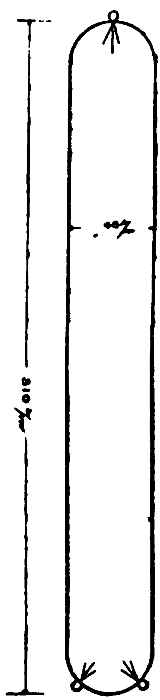
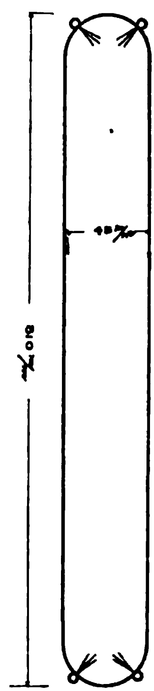


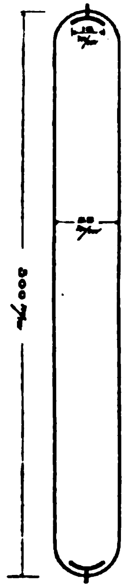
FIG. 8A.



Experiment 9.

A plain tube (see fig. 9) has placed at the ends cup electrodes, about 16 mm. in diameter, close on the glass. In appearance the

Fig. 9.



electrodes are like two small concave mirrors. When the current is passed, the effects produced are somewhat similar to a "radiant-matter tube," *i.e.*, the light produced by the discharge converges and the tube is generally lit up. The current is now diminished, and at a certain point bands make their appearance.

Experiment 10.

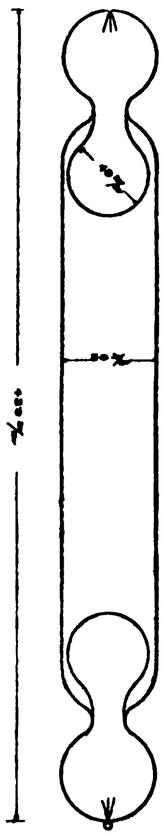
Following the last experiment, I tried a number of tubes constructed to show Professor Crookes' radiant matter phenomena. I find that if the current is sufficiently diminished the streams of light break up into bands. I am not aware whether this circumstance is known.

It ought to be mentioned that in some cases the effects can only be observed by passing a considerable quantity of current through the tube and then turning off the alternator, so that it runs slower and slower until it stops. The light in the tubes gradually diminishes until they are dark. Many effects can only be observed at the moment of extinction. Some tubes, in which it appears impossible to show the bands, give the phenomenon most distinctly at the instant referred to.

Experiment 11.

Tubes are employed as shown in fig. 11. They consist of plain tubes, the ends of which are expanded into bulbs that contain the

Fig. 11.



electrodes. Glass balls are blown on the contracted portions, so that these glass balls become the electrodes for the tube proper wholly and solely by induction, there being no connexion between the glass tube proper and the electrodes, except through the glass of these balls. Again the effects produced are the same as if the current were passed without the inductive process, *i.e.*, the bands are produced in a similar manner.

Experiment 12.

It has already been said that the glass itself appears to assist in the formation of the bands. This may be due to the electrification of the glass or to some other circumstance. In any case the bands have a tendency to stick to the glass with some pertinacity. The most convincing way to show this is by means of a tube lit up by induction in a manner similar to the type of tube employed in the experiment last described. A tube is employed with inductor electrodes consisting of glass balls, but the main tube consists of one contracted near the glass balls and then expanded, in the centre of its length, into a large sphere (see fig. 12). When such a tube is lit up the results must be looked for in the large glass sphere. It will be noticed that bands are formed at the entrances to the sphere, somewhat parabolic in shape, *i.e.*, the centres of the bands appear driven towards the centre of the sphere, while the edges of the bands seem to stick to the glass and to be retarded. On increasing the current the bands expand somewhat, and become slightly more numerous, and eventually a few of the most forward ones suddenly leave the glass sides and agree with the equatorial plane. (Figs. 12A and 12B give a rough idea of the two stages.)

Experiment 13.

To further illustrate the action of the glass in the formation of bands, a long tube is employed with a number of bulbs blown along the length of the tube at short distances apart. (See fig. 13.) On passing the current, bands are formed along the whole length of the tube, excepting at those places where the spheres are blown. At these places the bright bands disappear. If the current is considerably increased, the bright bands enter a certain distance into the spheres. These are not independent bands, but those driven out of the straight portions of the tube (the bands having become expanded), which is very easily observed when adjusting the current and watching the tube.

Experiment 14.

Professor Fleming suggested that, in order to make some of the experiments more convincing, the open tube and tube with a disk should be combined.

I therefore employed a tube shown in fig. 14 for this purpose. The tube is a plain tube, and at one end, 40 mm. from the electrode, a glass disk is hinged from the side of the tube. This disk may lie flat on the tube or stand vertical (as shown in the diagram) at pleasure, by tilting the tube or by turning it upon its axis. This tube must be placed horizontally in order to raise or lower the disk.

Fig. 12.

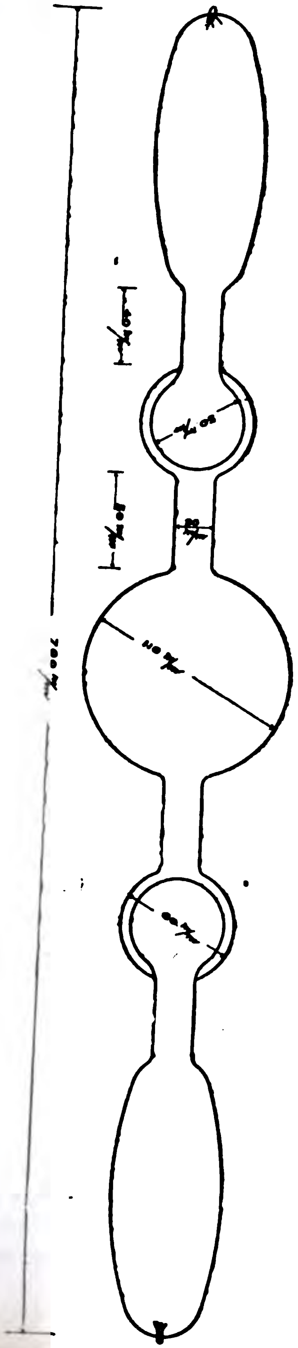
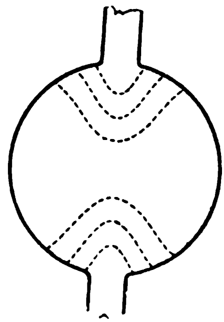
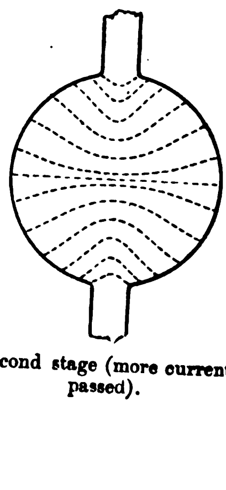


Fig. 12a.



First stage.

Fig. 12b.



Second stage (more current passed).

Fig. 13.

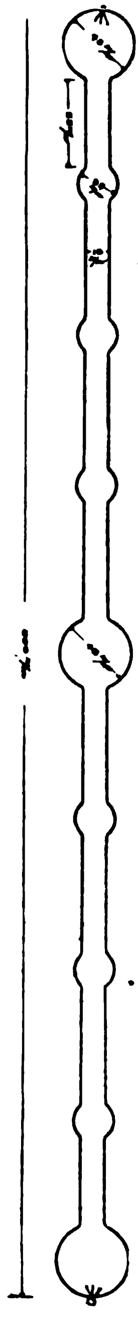
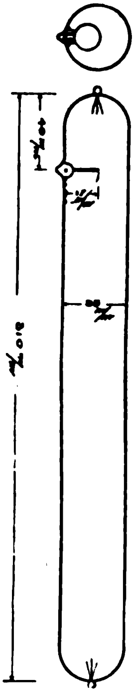


Fig. 14.



The phenomena of a plain tube are shown when the disk is down, and when up the regular bright bands are produced. This experiment is very striking.

IV. "On an Instrument for Indicating and Measuring Difference of Phase between E.M.F. and Current in any Alternating Current System." By Major P. Cardew, R.E. Communicated by Lord Kelvin, P.R.S. Received June 21, 1894.

If the periodic time of an alternating E.M.F be T, and if, owing to the presence of capacity or self-induction, or both in the circuit, the current passes through the value O at times differing from the times of passage of the E.M.F. through the same value by t, the electrical power will be $V \times C \times \cos(2\pi t/T)$, where V indicates the effective volts and C the effective current.

If a momentary contact be made at intervals exactly synchronising with the period of the alternating E.M.F. to complete the circuit of a suitable and suitably connected galvanometer, and if the time of

occurrence of this contact can be adjusted to any instant of the period T, the instant of passage of the alternating E.M.F. or current through O from positive to negative, or *vice versâ*, can be accurately determined.

The contact need not be absolutely momentary if the needle of the galvanometer have inertia sufficient, nor need it occur in each period, provided that the recurring period be an exact multiple of the alternating period.

On the principles ennunciated above, the following simple apparatus has been devised for the exact measurement of the angle $2\pi t/T$:—

A cylinder of boxwood or ebonite is caused to rotate synchronously with the alternating current generator, making one revolution to a complete period, either by direct connexion of its axle with that of the machine through suitable multiplying gear, in a manner similar to that used for the ordinary velocimeter, or by driving it by a synchronising motor.

In the surface of the cylinder is embedded one metal wire or strip parallel with the axle, and connected to the axle or to a contact ring.

Two insulated springs press against the surface of the cylinder; one of these, called the Volt brush V, is attached to a dial face accurately marked with degrees, centred at the axis of revolution, and capable of rotation round this axis, and provided with clamping and slow motion screws; the other brush, called the current brush C, is attached to an index moving over the face of the dial, and also provided with clamping and slow motion screws. For very exact measurement the index may carry a vernier.

The brushes are so arranged that they make simultaneous contact with the wire on the cylinder when the index is exactly at the zero of the dial, and the cylinder is rotated. This is tested by means of a battery and galvanometer, and the brushes are provided with suitable means of adjustment. If the wire is of appreciable width, the adjustment of the brushes should be such as to give maximum deflection on the galvanometer.

The connexions are as follows:—One terminal of the alternator is connected to the axle or contact ring of the cylinder by means of an ordinary rubbing contact; Brush V is connected to a sensitive dead-beat galvanometer, which can be shunted at will, and coils of sufficient impedance to enable the shunted galvanometer to withstand the full E.M.F., and to the other terminal of the alternator; Brush C is connected through a resistance which can be cut out of circuit to a low resistance galvanometer, and thence to a point on the main connected with the axle of the cylinder at a short distance from this connexion, so that a short piece of main is a shunt to this galvanometer and contact.

The *modus operandi* is first to adjust the dial and Brush V until Galvanometer V remains at zero, then adjust the index and Brush C until Galvanometer C remains at zero. The angle indicated is then exactly $2\pi t/T$, measuring the difference of phase between E.M.F. and current.

It will be seen that as this is a null method, the self-induction of the galvanometer circuits does not affect the results.

V. "On the difference of Potential that may be established at the Surface of the Ground immediately above and at various distances from a buried mass of Metal charge from a High Pressure Electric Light Supply." By Major CARDEW, R.E., and Major BAGNOLD, R.E. Communicated by LORD KELVIN, P.R.S. Received June 21, 1894.

On the 8th January, 1894, an accident occurred at Bournemouth of an unusual nature. An omnibus was in the act of drawing up in the roadway outside the Imperial Hotel, when the horses suddenly fell down, and one of them died in a few minutes.

All the men who assisted in extricating the horses felt tingling sensations in their limbs suggestive of electrical shock, and the connexion from the mains of the Bournemouth Electric Light Company to the hotel was known to pass underneath the spot at which the accident occurred.

This Company use the high pressure alternating system at 2,000 volts pressure, and in the case of this hotel the transformers were installed upon the premises.

On investigation, a defect in the insulation of one of the high pressure service lines was discovered, from which sparking had evidently taken place to the enclosing $1\frac{1}{2}$-in. wrought iron pipe.

This pipe was 32 ft. long, laid at a depth of about 18 in., and terminated at a brick junction box under the roadway, and a brick and cement area wall at the hotel. The ends were thus fairly insulated, while the rest of the pipe was in contact with the earth.

The accident took place during the progress of a thaw, after a very severe frost.

Upon consideration of the case, Major Cardew reported to the Board of Trade that in his opinion the accident was caused by leakage from the short length of charged pipe, affecting the potential of the surface of the ground to such an extent that between the fore and hind feet of the horse a sufficient difference of potential was established to give rise to the current which proved fatal.

At the same time, as the fact of such a result following from a simple contact with ordinary road material was a new experience of

the possible dangers attending the commercial supply of electrical energy, it was thought desirable to repeat the conditions as nearly as possible, and measure the results obtained.

As such an experiment could not, for obvious reasons, be satisfactorily carried out in London, it was arranged that it should be tried at Chatham, where the necessary apparatus was available.

A first experiment was made on the 26th January, 1894. In this a length of 33 ft. 3 in. of 2-in. iron pipe was buried to a depth of 18 in. within the salient of one of the demi-bastions at St. Mary's Barracks, Chatham.

A piece of well insulated 37-strand No. 16 cable was inserted in this pipe so that the ends of the conductors made good contact with the inside of the pipe at its centre point.

Over the centre of pipe two 56-lb. weights were placed on the surface of the ground, and at a distance of 4 ft. laterally another pair of 56-lb. weights were similarly placed.

A 16-kilowatt alternator was connected by means of an underground cable laid in stoneware pipes to the piece of cable in the 2-in. pipe.

The other pole of the alternator was connected to a 1½-in. service pipe in connexion with the barrack water mains. One end of this service pipe was in connexion with a hydraulic ram set in the ground at a distance of 140 ft. from the piece of buried pipe.

An 800-volt. Thomson static voltmeter was connected across the terminals of the alternator, and a pair of leads, well insulated, were attached by means of screws to the two pairs of weights situated over the buried pipe. These leads were run back together to the experiment room, and were connected alternately to a 140-volt static voltmeter and a Cardew voltmeter.

The voltage of the alternator was varied by means of the exciting current which was taken from accumulators.

The readings on both voltmeters are given in Table I.

As the earth resistance was rather low, the alternator could not be run up above 400 volts without overheating the armature.

Fig. 1 is a diagram of the arrangement.

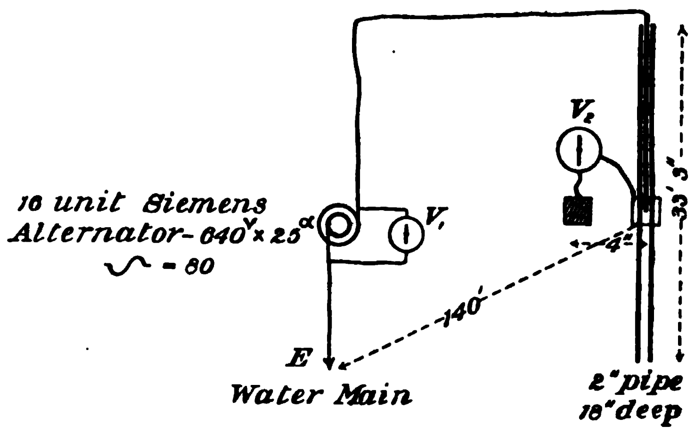
The results show a potential difference between points 4 ft. apart on the surface of over 20 per cent. of the whole P.D. used to charge the pipe when measured statically, and even when a current of nearly 1/10 ampère was drawn from the ground through a Cardew voltmeter the P.D. was more than 10 per cent. of the whole.

The propinquity of the scarp walls, however, appeared likely to have, to some extent, influenced the direction of the lines of flow of current, and it was determined to repeat it, changing the position of the pipe and extending the tests to a greater distance from the pipe. The second experiment was carried out on the 16th March, 1894.

Table I.—Experiment of 26th January, 1894.

Readings of statical voltmeter on terminals of alternator.	Readings across weights.		Remarks.
	On Cardew voltmeter.	On statical voltmeter.	
294	—	62	Resistance of earth circuit before run 17 ohms.
290	—	61	
305	30		
310	30		Distance between weights 4 ft.; weights, each, 2/56 lbs.
375	38		
380	40		
400	43		Soil loamy and very wet.
390	—	83	
375	—	81	About 80 periods per second.
385	—	82·5	
390	—	83	
394	—	84	
400	—	86	
398	—	85	
400	—	86	
403	—	87·5	

Fig. 1.

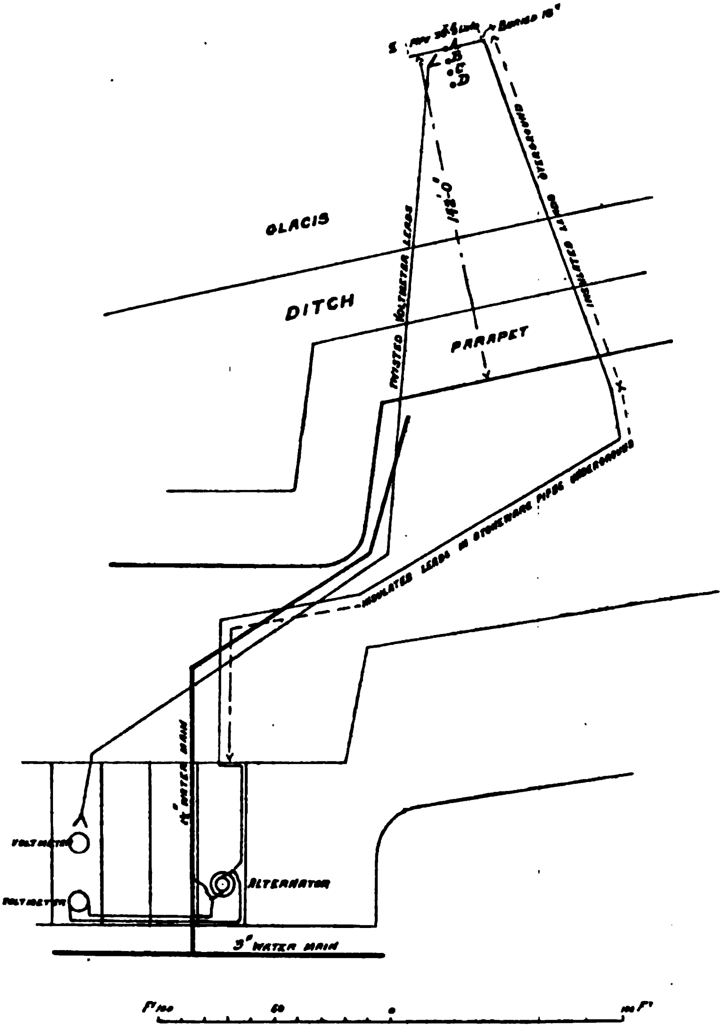


The arrangements are shown on fig. 2, and the results given in Table II.

An interesting note made on this experiment is as follows:—

"The soil in which the pipe was buried was loamy, and when digging the trench no worms were noticed, but towards the conclusion of the experiment it was observed that hundreds of worms came to the surface between 3 ft. and 9 ft. on either side of the pipe; no worms appeared, however, immediately over the pipe."

Fig. 2.



Scale $\frac{1}{500}$

The ground was much drier than was the case in the first experiment, and, probably in consequence of this, it appears that while the statical P.D. between the two first weights is relatively greater in the second than in the first experiment, the P.D. obtained with a current-using instrument is relatively less.

The statical P.D.'s to be observed extended rather beyond the range of one voltmeter. Thus, between weights A and B no statical

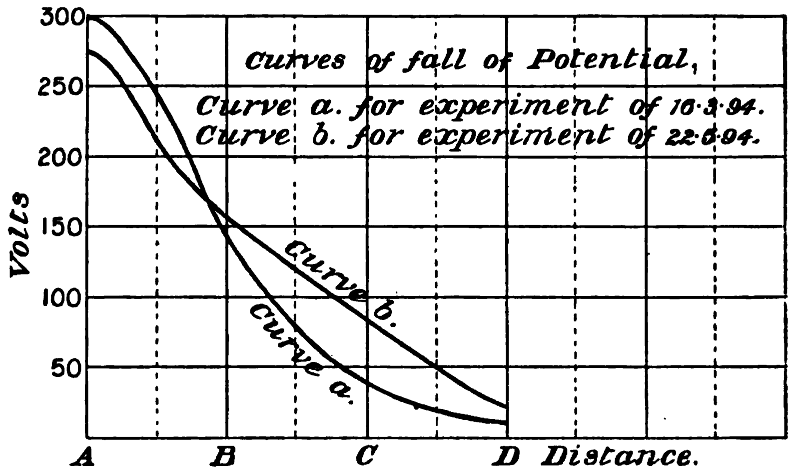
Table II.—Experiment of 16th March, 1894.

Readings on statical voltmeter on terminals of alternator.	Readings across weights. On Cardew voltmeter.	Readings across weights. On statical voltmeter.	Remarks.
	Weights A and B.		
340	—	105	Weights, each 1/56 lbs.
425	—	130	
440	—	140	Frequency about 80.
408	28	—	Weights 4 ft. 5 in. apart from centre to centre in a line at right angles to pipe, and about the centre of its length.
445	29·5	—	
485	33	—	
515	36	—	
550	37·5	—	
590	42	—	
610	43	—	
620	43·5	—	
	Weights B and C.		
575	—	102	Pipe buried to same depth as in first experiment.
590	—	106	
600	—	108	
610	—	110	Soil rather dry.
610	28·5	—	
	Weights C and D.		
610	—	15	
	Weights B and D.		
610	37·5	—	
610	—	135	
	Weights A and C.		
505	55	—	P.D. too large for statical voltmeter.
520	55·5	—	
550	56	—	
590	60	—	
600	60·5	—	
610	60·75	—	
	Weights A and D.		
550	64	—	P.D. too large for statical voltmeter.
570	66	—	
600	70	—	
610	70·75	—	
620	71·5	—	
620	73	—	

observations could be taken beyond a charging pressure of 440 volts, while between weights C and D the P.D., even with 610 volt charging pressure, was too small for exact measurement.

By interpolating an approximate value for the fall of potential between A and B from the readings obtained on the Cardew voltmeter, a rough curve of the fall of potential on the surface of the ground for a charging pressure of 610 volts is obtained, as shown in fig. 3, Curve *a*, the height of the curve above absolute zero being

FIG. 3.



made consistent with the evident approach of the curve to its asymptote at D. In this case, therefore, the effect of the leakage extended to a distance of about 14 ft. from the pipe, and the fall of potential on the surface appears to have been most rapid at a distance from the line immediately above the pipe equal to twice the depth at which it was buried.

A third experiment was carried out on the 22nd May, 1894, with the same arrangements as in the second experiment. The objects for which this was undertaken were chiefly to obtain a record of the variation of P.D. on the surface with the charging current, which had not been previously recorded, to obtain results from charging with continuous, as well as alternating, current, and to extend the investigation as regards the latter.

Unfortunately, the continuous current generator could not be used, owing to a mechanical defect, which could not be removed in the limited time available. Still, the results obtained with alternating current are, it is considered, of some value, especially as the condition of the ground was different from that which existed at the time of

either of the two former experiments, it being wet on the surface with rain falling during a portion of the time, and dry underneath.

Curve *b* in fig. 3 appears to show by its form the effect of this state of the soil, the fall of potential from B to D being approximately indicated by a straight line. That is to say that the current flow was practically entirely confined to the surface layer. The curve is deduced from the results of experiments shown in Table III for a charging pressure of 550 volts.

Table III.—Experiment of 22nd May, 1894.

No.	At machine.			Readings across weights.		Remarks.
	Volts.	Current.	Exciting current.	On Cardew voltmeter.	On statical voltmeter.	
				Weights A and B.		
1	300	13·3	—	—	65	Weather wet after
2	345	15·17	—	—	75	dry. Wet surface,
4	370	16·4	—	—	80½	dry underneath.
6	387	17	—	—	84½	Weights, each, 1/56
9	430	18·8	—	—	95	lbs., and arranged
10	500	21·7	—	—	107	as in second experi-
11	550	24·35	..	—	122½	ment.
12	610	27·75	—	—	135	Pipe buried to same
13	625	28·8	—	—	142	depth.
				Weights A and C.		
14	470	21·79	—	40	—	
15	505	22·1	—	43½	—	
16	550	25·53	—	47	—	
17	565	26·7	—	50	—	
18	610	28·6	—	56	—	
19	625	29	—	59	—	
				Weights A and D.		
20	495	23·11	12	55	—	
21	530	24·3	13·5	59½	—	
22	555	25·53	14·25	63½	—	
23	580	26·4	15·75	67½	—	
24	—	21·79	12	—	225	
25	—	24·35	13·5	—	245	
26	—	25·3	14 25	—	255	
27	—	26·4	15·75	—	262	

In order to obtain a direct indication of the statical P.D. between weights A and B, the voltmeter which had been used for recording

the charging pressure was connected to these points and observations taken, when the exciting current for the field magnets of the dynamo and the charging current to the pipe were practically the same as in a previous set of experiments, for which the charging volts were known. Both sets of experiments are shown in the table. The disturbance of the worms was again very distinct, and they appeared, on reaching the surface, to set their bodies parallel to the pipe, or along an equipotential line.

The experiments, as a whole, show conclusively that, under very different conditions of weather and amount of moisture in the soil, it is possible to produce a fall of potential amounting to 25 per cent. of the whole pressure in the supply between points on the surface of the ground 4 ft. apart, a distance which most horses stand over, and, further, that, although the connexion of such points by a conducting body naturally reduces this potential difference, yet, when this conductor has about the resistance of a horse (say 400 ohms), sufficient current will pass to give a severe shock.

It is hoped that, when opportunity serves, these experiments may be extended to the case of pipes buried to greater depths, and to observe whether any difference is produced by the use of a steady, in place of an alternating pressure.

VI. "On the Viscosity of Water as determined by Mr. J. B. Hannay by means of his Microrheometer." By Robert E. Barnett, A.R.C.S. Communicated by Professor T. E. Thorpe, F.R.S. Received May 6, 1894.

In a paper entitled "On the Microrheometer," published in the 'Philosophical Transactions' for 1879, Vol. 170, p. 275, Mr. J. B. Hannay describes an apparatus with which he made measurements of the rate of flow of water, and of some aqueous saline solutions through a capillary tube, and he deduces from the observations certain relations between the chemical nature of the salt dissolved, and its effect on the rate of flow of water.

Inasmuch as Mr. Hannay furnishes us with details of the dimensions of his apparatus, I have, at Professor Thorpe's suggestion, transformed his relative numbers into absolute measure of viscosity in order to compare his results with those of other workers. The data given are as follows:—

Diameter of capillary tube	=	0·0938 mm.
Length ,,	=	21 mm.
Capacity of glass bulb	=	4·0530 c.c.

The time of flow was measured by a stop-watch, and each number recorded was the mean of ten observations.

In calculating the value of the viscosity of water from Mr. Hannay's figures, the well-known formula

$$\eta = \frac{\pi r^4 pt}{8Vl} - \frac{V\rho}{8\pi lt}$$

was employed, in which—

η = Viscosity in dynes per sq. cm. in C.G.S measure.
r = Radius of the capillary in cm.
l = Its length in cm.
V = Volume of liquid transpired in c.c.
ρ = Its density at the temperature of observation.
p = Pressure in dynes per sq. cm. (g = 981).
t = Time of flow in seconds.

On applying this formula to the experimental results given for water, the figures embodied in the subjoined table (I) were obtained. In Columns 1 and 2 are the temperatures of observation and the corresponding times of flow as recorded by Mr. Hannay. Columns 3

Table I.

1.	2.	3.	4.	5.
T°.	t.	$\frac{\pi r^4 p}{8Vl} \cdot t$.	$\frac{V}{8\pi l} \cdot \frac{\rho}{t}$.	η.
0	235·0	0·000514	0·000327	0·000187
1	220·8	0·000483	0·000348	0·000135
2	211·7	0·000463	0·000363	0·000100
3	205·0	0·000449	0·000375	0·000074
4	198·5	0·000434	0·000387	0·000047
5	192·5	0·000421	0·000399	0·000022
6	187·1	0·000409	0·000410	−0·000001
7	181·0	0·000396	0·000424	−0·000028
10	167·2	0·000366	0·000459	−0·000093
15	146·0	0·000320	0·000526	−0·000206
20	131·3	0·000287	0·000584	−0·000297
25	115·5	0·000253	0·000663	−0·000410
30	106·0	0·000232	0·000721	−0·000489
35	96·8	0·000212	0·000789	−0·000577
40	88·7	0·000194	0·000859	−0·000665
45	81·8	0·000179	0·000930	−0·000751
50	75·5	0·000165	0·001005	−0·000840
60	65·0	0·000142	0·001162	−0·001020
70	57·5	0·000126	0·001306	−0·001180
80	49·8	0·000109	0·001499	−0·001390
85	47·1	0·000103	0·001579	−0·001476
90	45·5	0·000100	0·001630	−0·001530
95	44·3	0·000097	0·001668	−0·001571
100	43·8	0·000096	0·001681	−0·001585

and 4 contain the calculated values for the first and second parts of the formula respectively, and in Column 5 are the values for the viscosity obtained by subtracting the figures in Column 4 from those corresponding to them in Column 3.

On comparing these results with the values given by Poiseuille, Slotte, Sprung, and Thorpe and Rodger as tabulated below (II), it will be seen that Mr. Hannay's observations yield discordant, and, indeed, utterly absurd, values for the viscosity of water. At 0°, for example, the viscosity would appear to be below that of any known liquid, and at 6° it becomes *nil*.

Table II.—Water.

Viscosity Coefficients in Dynes per sq. cm. between 0° and 100°.

Temperature.	Poiseuille.	Sprung.	Slotte.	Thorpe and Rodger.
0	0·01776	0·01778	0·01808	0·01778
10	0·01309	0·01301	0·01314	0·013025
20	0·01008	0·01003	0·01008	0·010015
30	0·00803	0·00802	0·00803	0·007975
40	0·00653	0·00657	0·00657	0·006535
50	..	0·00553	0·00553	0·005475
60	..	..	0·00472	0·00468
70	..	..	0·00408	0·00406
80	..	..	0·00358	0·00356
90	..	..	0·00318	0·003155
100	..	..	0·00285	0·00283

As a matter of fact, it is physically impossible to pass a volume of water such as Mr. Hannay employs under a pressure of 1 m. of water through a capillary of the dimensions given in the time recorded. At 20°, for instance, the time of flow required under these conditions would be about 4600 seconds instead of 131·3 seconds as stated.

I have carefully examined Mr. Hannay's data with the object of discovering any slip or misprint which might satisfactorily account for the discrepancy. Thus, I have tried the effect of substituting "centimetre" for "millimetre," and "radius" for "diameter" in the dimensions given, but no alteration of the kind could be made to yield values for η agreeing with those of other observers.

In the light of these results, it would seem to be premature to discuss Mr. Hannay's observations on saline solutions, or to criticise the generalisations he deduces from them.

VII. "The Rotation of the Electric Arc." By ALEXANDER PELHAM TROTTER, B.A. Communicated by SILVANUS P. THOMPSON, F.R.S. Received June 12, 1894.

In the course of experiments made with the view of realising as a practical standard of light, the method of using one square millimetre or other definite area of the crater of the positive carbon of an electric arc,* the author has found that the effective luminosity is not as theory would predict,† either constant or uniform. By the use of a double Rumford photometer, giving alternating fields, as in a Vernon Harcourt photometer, his attention was called to a bright spot at or near the middle of the crater. The use of rotating sectors accidentally revealed that a periodic phenomenon accompanied the appearance of this bright spot, and although it is more marked with a short humming arc, the author believes that it is always present.

An image of the crater was thrown on a screen by a photographic lens; and a disc having 60 arms and 60 openings of 3°, and rotating at from 100 to 400 revolutions per minute, was placed near the screen. Curious stroboscopic images were observed, indicating a continually varying periodicity seldom higher than 450 per second, most frequently about 100, difficult to distinguish below 50 per second, and becoming with a long arc a mere flicker. The period seemed to correspond with the musical hum of the arc, which generally breaks into a hiss at a note a little beyond 450 per second. The hum is audible in a telephone in the circuit, or in shunt to it. The current was taken from the mains of the Kensington and Knightsbridge Electric Light Company, often late at night, after all the dynamos had been shut down. The carbons were, of course, not cored; six kinds were used.

A rotating disc was arranged near the lens, to allow the beam to pass for about 1/1000th of a second, and to be cut off for about 1/100th of a second. It was then found that a bright patch, occupying about one quarter of the crater, appeared to be rapidly revolving. Examination of the shape of this patch showed that it consisted of the bright spot already mentioned, and of a curved appendage which swept round, sometimes changing the direction of its rotation. This appendage seemed to be approximately equivalent to a quadrant sheared concentrically through 90°. Distinct variations in the luminosity of the crater are probably due to the fact that this is only an approximation.

* J. Swinburne and S. P. Thompson, discussion on paper by the author, 'Inst. Electrical Eng.,' vol. 21, pp. 384 and 403.

† Abney and Festing, 'Phil. Trans.,' 1881, p. 890; S. P. Thompson, 'Soc. Arts Journ.,' vol. 37, p. 332

The *a priori* theory of the constant temperature of the crater is so attractive, that the author is inclined to attribute this phenomenon, not to any actual change of the luminosity of the crater, or to any wandering of the luminous area, as is seen with a long, unsteady arc, but to the refraction of the light by heated vapour. All experiments, such as enclosing the arc in a small chamber of transparent mica, or the use of magnets, or an air blast, have failed to produce any effect. A distortion of the image of the crater while the patch revolves, has been looked for, but nothing distinguishable from changes of luminosity has been seen.

An unexpected difficulty is thus introduced in the use of the arc as a standard of light, and one which may interfere with its use under some circumstances as a steady and continuous source of light. The author is further examining this phenomenon, with the view of ascertaining its nature, and of finding practical conditions under which it is absent or negligible.

VIII. "The Electric Strength of Mixtures of Nitrogen and Hydrogen." By Miss P. G. FAWCETT. Communicated by Professor J. J. THOMSON, F.R.S. Received June 21, 1894.

The experiments described in this paper were undertaken at Professor Thomson's suggestion, and have been carried out with the advantage of his advice and help.

The immediate object of the experiments was to determine the electromotive force required to produce a spark between two flat parallel metal plates in a mixture of hydrogen and nitrogen in different proportions and at different pressures.

The hydrogen used was obtained by electrolysis of water, as it was found that that obtained in the ordinary way from zinc and hydrochloric acid was liable to contain impurities which seriously affected its electric strength.

The two gases were collected over water in a graduated cylindrical gas-holder, and were allowed to stand for some hours to give them time to mix before being put into the apparatus. The mixture was passed through sulphuric acid, and also through cotton wool to remove dust.

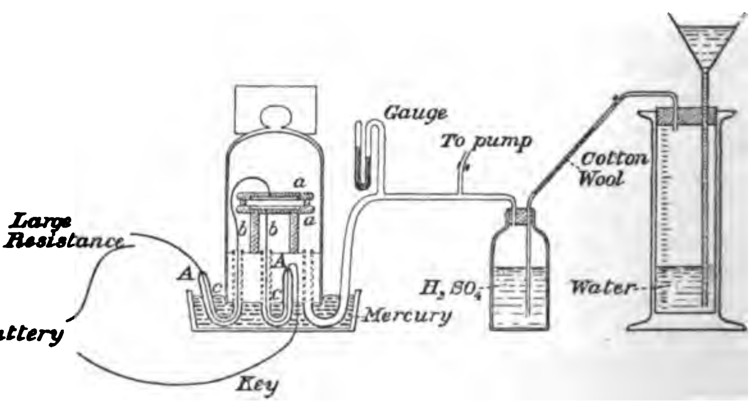
The electromotive force was supplied by a battery of storage cells, each of about 2 volts, and was measured simply by counting the number of cells. The strength of the cells was measured by a quadrant electrometer.

At very low pressures it was found that, unless special precautions were taken to prevent the discharge passing anywhere except between

the opposed faces of the plates, it would take a longer path, and pass between the connecting wires on the backs of the plates. When the distance was 0·047 in., the discharge began to pass between the backs of the plates when the pressure was reduced to about 2 mm.

In the later experiments this was prevented by using plates embedded in ebonite discs, only leaving exposed the faces between which the spark was intended to pass, and by making the connexions with indiarubber covered wires. Even then, after the plates had been used for some time, the indiarubber showed signs of giving way, and a discharge occasionally passed partly between the wires and partly between the plates.

The plates were kept at the right distance apart by placing between them small flat pieces of ebonite of the same thickness (0·047 in.). In the earlier experiments, the plates were in an ordinary bell-jar standing on a flat surface, the rim being greased with a mixture of bee's-wax and vaseline. Thinking it possible that there might be some vapour given off by the grease, I arranged the apparatus so that it could be made air-tight without grease. For this purpose I used a rather narrow bell-jar, closed at the bottom by an indiarubber stopper, through which passed three glass tubes for conveying the connecting wires, and for communicating with the air-pump and the gasholder. The jar, with its stopper, was placed in a vessel containing mercury, so that the junction of the glass and indiarubber was immersed, the tubes being bent so that their ends came above the mercury. The arrangement is shown in the accompanying figure; *a*, *a* are the ebonite discs in which the plates are embedded; the wires,



b, *b*, pass through the ebonite, and are covered with indiarubber throughout their length until they come out into the open air at the ends, A, A, of the tubes, *c*, *c*, which are sealed with sealing-wax.

1 : 0.		2 : 1.		3 : 2.		1 : 1.		2 : 3.		1 : 2.		0 : 1.	
Pr.	E.M.F.	Pr.	E.M.F.	Pr.	E.M.F.	Pr.	E.M.F.	Pr.	E.M.F.	Pr.	E.M.F.	Pr.	E.M.F.
24	586	15	466	17½	508	14½	432	18½	480	27	536	25	488
15½	480	10	388	12	436	10	396	14½	444	17	476	17	456
9½	406	7½	368	8	400	7	378	11½	424	11	450	10½	442
5½	366	5½	370	5	396	5	382	9½	404	7	428	7½	460
3¼	366	3½	386	2¾	774	3	428	6½	380	4	440	4½	700
1½	1400	2¾	470	2¼	1200	2	560	4	462	2	720	1½	980
13¼	460	1¾	800			22	500	2	above 1600	1½	940	17½	440
10	426					16	458					9½	420
7	370					10	416					5½	500
4¼	366					3½	520						
2	1400					14½	452						
						8	416						
						2½	552						
						2	above 1180						

The figures at the top give the ratio of the volume of nitrogen to that of hydrogen.

The results (p. 265) were obtained when no grease* was present, and the spark was not able to pass except between the opposed surfaces of the plates.

Distance between the plates = 0·047 in.

These results are represented by the continuous curves figs. 1—7, in which the abscissæ represent the pressure in millimetres of mercury, and the ordinates the E.M.F. in volts. The dotted curves in the same figures represent the means of the results of several series of observations with the earlier arrangement of the apparatus, in which grease was present, and no precautions were taken to prevent the discharge passing otherwise than between the plates.

Fig. 1.

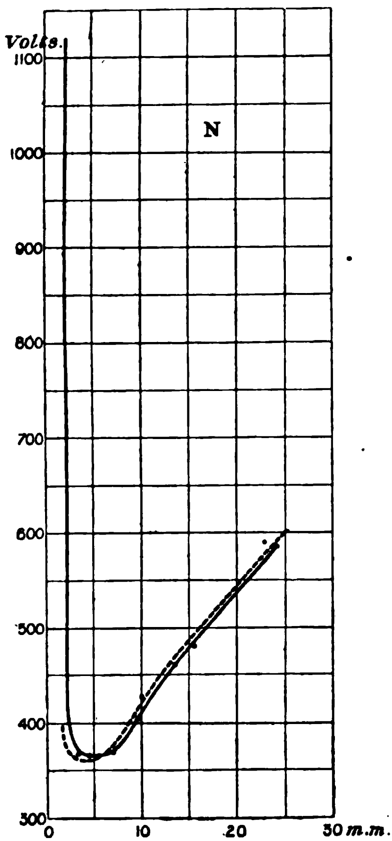
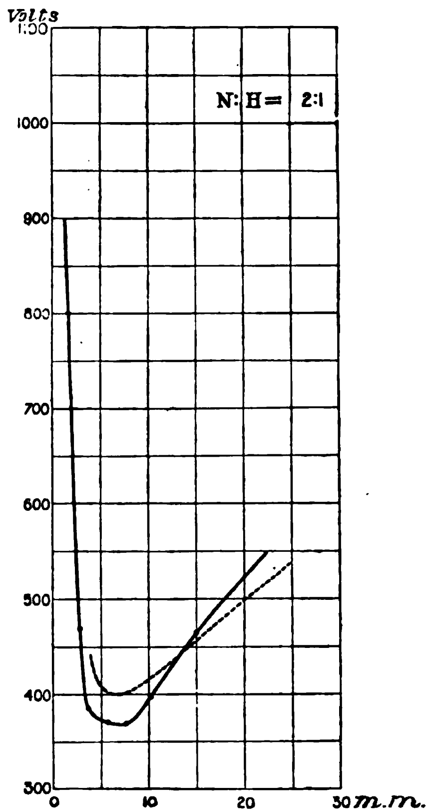


Fig. 2.



* There was always a small amount of grease on the stop-cock of the air-pump, but it does not seem probable that such a small quantity would have an appreciable effect.

Fig. 3.

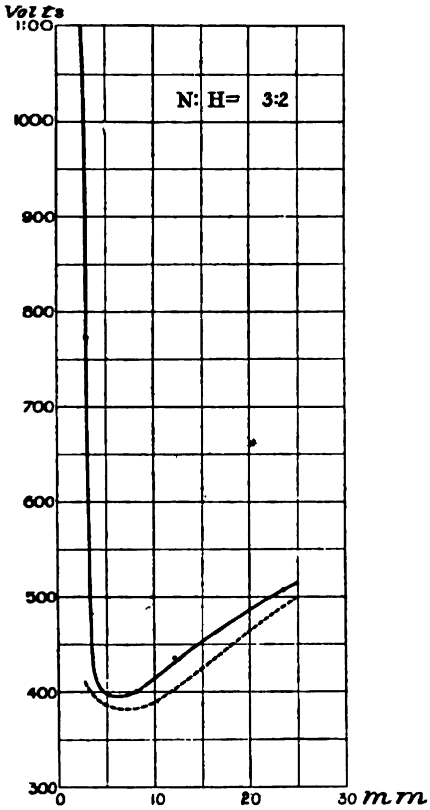

Volts
1100
1000
900
800
700
600
500
400
300
0
10
20
30 m.m
N: H= 3:2

Fig. 4.

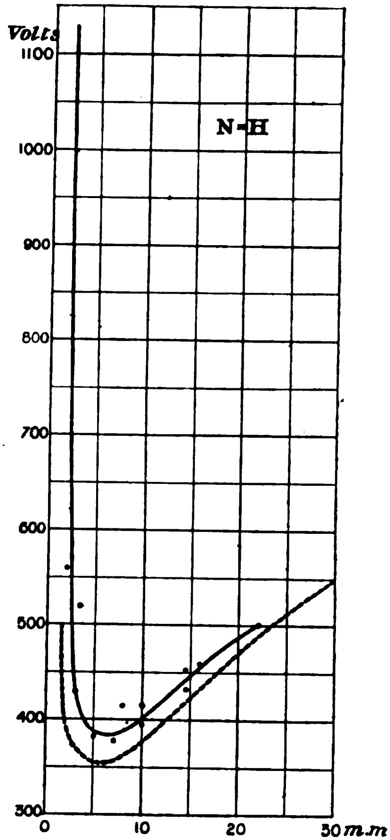

Volts
1100
1000
900
800
700
600
500
400
300
0
10
20
30 m.m
N=H

Fig. 5.

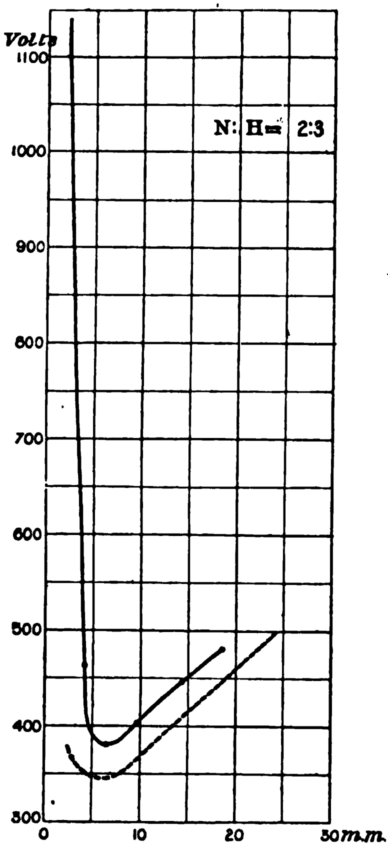

Volts
1100
1000
900
800
700
600
500
400
300
0
10
20
30 m.m.
N: H = 2:3

Fig. 6.

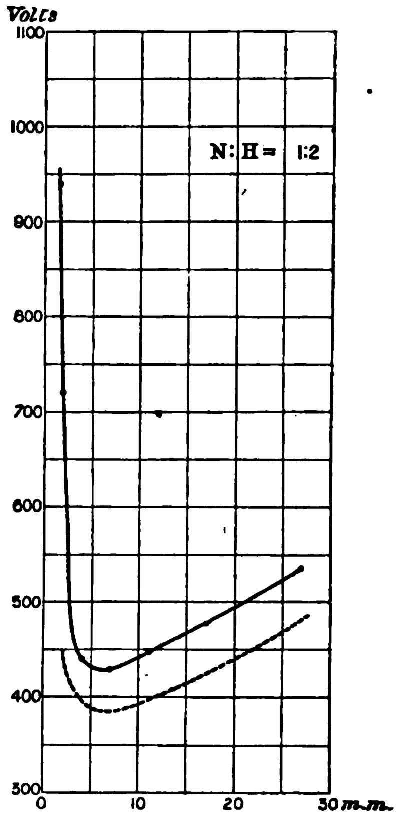
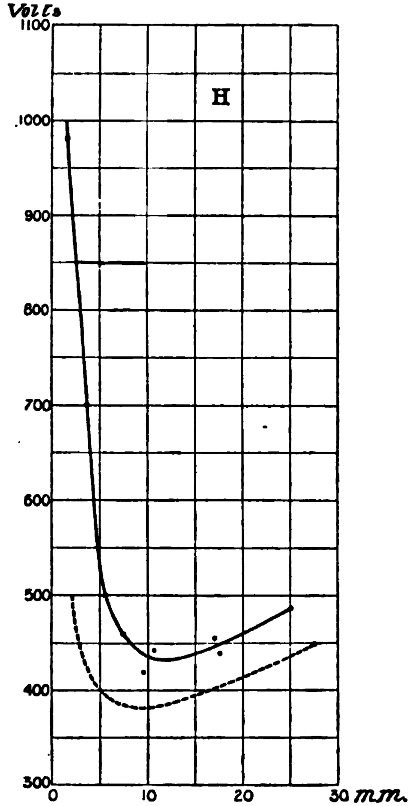

Volts
1100
1000
900
800
700
600
500
400
300
0
10
20
30 m.m.
N: H = 1:2

Fig. 7.



These curves are not traced for pressures lower than about 2 mm. The two curves are nearly identical in the case of pure nitrogen; in the other cases, with one exception, that of N : H = 2 : 1, the discharge seems to pass more easily when grease is present.

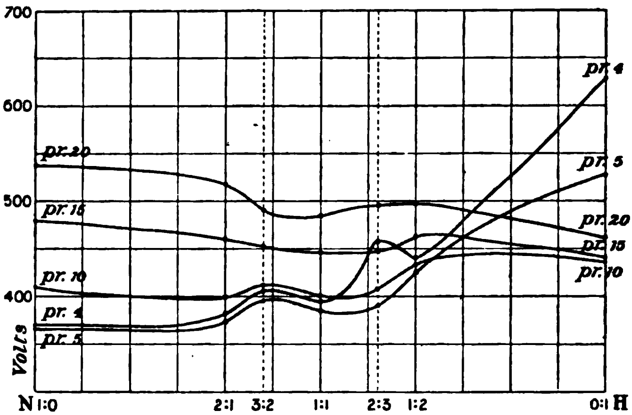
It will be noticed that at fairly high pressures the E.M.F. required to produce a spark diminishes nearly uniformly as the pressure diminishes, but that there exists for each mixture a critical pressure at which the E.M.F. is a minimum, and that as the pressure diminishes from the critical value the E.M.F. continually increases. When the pressure is slightly less than the critical pressure the E.M.F. increases with remarkable rapidity as the pressure diminishes. In fact, as the pressure falls by about ½ mm. the E.M.F. may increase by several hundred volts.

The critical pressure diminishes as the proportion of nitrogen to

hydrogen is increased. With spark length 0·047 in. it varies from about 11 mm. in pure hydrogen to about 5 mm. in pure nitrogen.

At high pressures the E.M.F. required to produce a discharge at a given pressure diminishes continually as the proportion of hydrogen to nitrogen is increased. At low pressures the relation between the composition of the mixture and the E.M.F. required to produce a spark at a given pressure is less simple. It is represented by the curves in fig. 8, where the ordinates give the E.M.F. in volts, and the ratio in which the ordinate divides the line NH is equal to the ratio of the volumes of nitrogen and hydrogen in the mixture.

FIG. 8.



The curves are given for pressures of 20, 15, 10, 5, and 4 mm. They could not be considered accurate for lower pressures, owing to the extreme steepness of the curves (1—7), in consequence of which the E.M.F., which will produce a spark at a given low pressure, cannot be determined with any precision.

It will be seen that at the pressures 15 and 20 mm. the general slope of the curves is downwards from the nitrogen end to the hydrogen end, but that at pressures 10, 5, 4 the slope is the other way, showing that at low pressures the effect of introducing more hydrogen is in general to increase the electric strength of the mixture.

But as the proportion of nitrogen to hydrogen increases from 0 : 1 to 1 : 0, the electric strength does not diminish uniformly, but it may pass through one or more maxima and minima.

It is, perhaps, hardly necessary to say that the curves in fig. 8 cannot be regarded as accurate for strengths of mixture intermediate between those at which the observations were actually made. The

curves, especially at low pressures, must be considered rather as a convenient way of showing which dots in the figure correspond to any given pressure than as an attempt at interpolation.

IX. "The Asymmetrical Probability Curve." By F. Y. Edgeworth, M.A., D.C.L. Communicated by Sir G. G. Stokes, F.R.S. Received June 14, 1894.

(Abstract.)

The asymmetrical probability curve is the second approximation—the symmetrical probability curve being the first approximation—to the law of frequency which governs the set of values assumed by a function of numerous independently fluctuating small quantities. The curve may be written

$$y = \frac{1}{\sqrt{\pi c}}\,{}^{\frac{-x^2}{c^2}}\left[\;-\frac{2j}{c^3}\left(\frac{x}{c}-\frac{2}{3}\frac{x^3}{c^3}\right)\right];$$

where $y\Delta x$ is the number of errors occurring between x and $x+\Delta x$, $c^2/2$ is the mean square of errors, and j is the mean cube of errors—errors measured from the centre of gravity. This form is obtained by completing the analysis which Todhunter, after Poisson, has indicated ('History of Probabilities,' Art. 1002); and independently by obtaining a general form for the asymmetric probability curve, and deducing therefrom the Poissonian formula in the case when the asymmetry is slight—the only case to which that formula is applicable.

Among the peculiarities of the asymmetric probability curve are the want of coincidence between the arithmetic mean and the position of the greatest ordinate, and the descent of the curve at one extremity below the abscissa—the ordinate appearing to denote *negative* probability.

An important case of the general curve is afforded by the *Binomial*, for which each of the independent elements admits of only *two* values. The approximate form of the Binomial, obtained directly by Laplace (Todhunter, 'History,' Art. 993), is deducible from the general theory. The general, or multinomial, probability curve can always be represented by a binomial.

The principle of the asymmetric probability curve affords an extension of the theory of *correlation* investigated by Messrs. Galton and Hamilton Dickson ('Roy. Soc. Proc.,' 1886, p. 63). The symmetrical probability surface

$$z = \frac{1}{\sqrt{\pi}\sqrt{1-r^2}}\,e^{\frac{-(x^2-2rxy+y^2)}{1-r^2}}$$

becomes now slightly distorted, so that the locus of the most probable y deviation, corresponding to an assigned x deviation is no longer a straight line, but a parabola.

X. "The Differential Covariants of Twisted Curves, with some Illustrations of the Application to Quartic Curves." By R. F. GWYTHER, M.A., Fielden Lecturer in Mathematics, Owens College, Manchester. Communicated by Professor HORACE LAMB, F.R.S. Received May 31, 1894.

(Abstract.)

The object of the earlier parts of this paper is to obtain relations connecting what Halphen* calls the canonical invariants of the curve, without the intervention of what are called by him the fundamental invariants. In any geometrical investigation it is the canonical invariants which present themselves, and the relations between the consecutive canonical invariants, and the values of their differential coefficients must, if the method of investigation is to be used, be expressed, in terms of canonical invariants only, without the intervention of the other series of invariants which Halphen treats as fundamental.

In the paper, the notation of Halphen's paper cited above is generally followed, but the mode of initial investigation, as in a previous paper on covariants of plane curves,† is made to depend upon a homographic transformation with infinitesimal arguments.

Writing ξ, η, ζ for the coordinates of a current point, x, y, z for the coordinates of a point on a standard curve, and y_n and z_n for $d^ny/dx^n \cdot n!$ and $d^nz/dx^n \cdot n!$, the arguments of a covariant function are shown to be

$$f = \xi - x,$$

$$g = z_3\{\eta - y - y_1(\xi - x)\} - y_3\{\zeta - z - z_1(\xi - x)\}/y_2z_3 - y_3z_2,$$

$$h = y_2\{\zeta - z - z_1(\xi - x)\} - z_2\{\eta - y - y_1(\xi - x)\}/y_2z_3 - y_3z_2,$$

with

$$a_n = y_2z_n - y_nz_2/y_2z_3 - y_3z_2,$$

$$b_n = y_nz_3 - y_3z_n/y_2z_3 - y_3z_2,$$

while $y_2z_3 - y_3z_2$ is an invariant which will not appear independently.

The other conditions that a function $\phi(f, g, h, a_n, b_n \ldots)$ may be a covariant function are (1) that it shall be isobaric, counting f, g, h, a_n,

* "Sur les Invariants Différentiels des Courbes Gauches," 'Journal de l'École Polytechnique,' cahier 47, vol. 28, 1880.

† 'Phil. Trans.,' vol. 184 (A), 1893, p. 1171.

and b_n of the weights -1, -3, -2, $n-3$, and $n-2$ respectively, and (2) that, if d be the algebraic degree of the function, then

$$\left\{ g\frac{\partial}{\partial f}+2h\frac{\partial}{\partial g}-\ldots\ldots-\mathrm{S}ma_m b_{n-m+1}\frac{\partial}{\partial a_n}-(\mathrm{S}mb_m b_{n-m+1}-2a_n)\frac{\partial}{\partial b_n} -\ldots\ldots\right\}\phi=0;$$

$$\left\{ h\frac{\partial}{\partial f} \qquad -\ldots\ldots-\mathrm{S}ma_m a_{n-m+1}\frac{\partial}{\partial a_n}-\mathrm{S}mb_m a_{n-m+1}\frac{\partial}{\partial b_n} -\ldots\ldots\right\}\phi=0;$$

$$\left\{ f^2\frac{\partial}{\partial f}+(fg+h)\frac{\partial}{\partial g}+fh\frac{\partial}{\partial h}-df-\ldots\ldots-(n-3)\,a_{n-1}\frac{\partial}{\partial a_n} -((n-2)\,b_{n-1}-a_n)\frac{\partial}{\partial b_n}-\ldots\ldots\right\}\phi=0;$$

$$\left\{ g\left(f\frac{\partial}{\partial f}+g\frac{\partial}{\partial g}+h\frac{\partial}{\partial h}-d\right)-\ldots\ldots-\mathrm{S}\,(m-1)\,a_m b_{n-m}\frac{\partial}{\partial a_n} -\mathrm{S}\,(m-1)\,b_m b_{n-m}\frac{\partial}{\partial b_n}-\ldots\ldots\right\}\phi=0;$$

$$\left\{ h\left(f\frac{\partial}{\partial f}+g\frac{\partial}{\partial g}+h\frac{\partial}{\partial h}-d\right)-\ldots\ldots-\mathrm{S}\,(m-1)\,a_m a_{n-m}\frac{\partial}{\partial a_n} -\mathrm{S}\,(m-1)\,b_m a_{n-m}\frac{\partial}{\partial b_n}-\ldots\ldots\right\}\phi=0.$$

These equations are then used to find the forms of the earlier invariants, and to show that there are only four independent covariants, which may be written

$$\zeta=h,\quad \eta=g-\frac{Q_6}{P_6}h,$$

$$\xi=f-\left(u_4+2\frac{Q_6}{P_6}\right)g-\left(u_5-\frac{Q_6^2}{P_6^2}\right)h,$$

$$\varpi=1-\left(2u_4+3\frac{Q_6}{P_6}\right)f-\left(3u_5-a_5+3b_4-3u_4\frac{Q_6}{P_6}-3\frac{Q_6^2}{P_6^2}\right)g -\left(v_5-3u_5\frac{Q_6}{P_6}+\frac{Q_6^3}{P_6^3}\right)h,$$

where
$$u_4 = a_4,$$
$$u_5 = a_5 - 3b_4 - a_4^2,$$
$$v_5 = b_5,$$
$$u_6 = a_6 - 3a_4b_4 - 2b_5,$$
$$v_6 = b_6 - a_4b_5 - 2b_4^2,$$
and
$$P_6 = u_6 - 3u_4u_5 - u_4^3,$$
$$Q_6 = v_6 + u_5^2.$$

The planes, whose equations are $\xi = 0$, $y = 0$, $\zeta = 0$, $\omega = 0$, form a tetrahedron, which will be called the canonical tetrahedron of reference at any point on the curve, and if we put

$$x = \frac{\xi}{\omega}, \qquad y = \frac{\eta}{\omega}, \qquad z = \frac{\zeta}{\omega},$$

the coefficients of the expansions of y and z in terms of x are the canonical invariants.

We write these expansions

$$y = x^2 \qquad\qquad + \beta_7 x^7 + \&c.,$$
$$z = x^3 \qquad + \alpha_6 x^6 + \alpha_7 x^7 + \ldots\ldots,$$

and since

$$g = f^2 + \ldots\ldots + b_4 f^4 + \&c.,$$
$$h = f^3 + \ldots\ldots + a_4 f^4 + \&c.,$$

we see that with respect to the canonical axes,

$$a_4 = b_4 = a_5 = b_5 = b_6 = 0.$$

To obtain the result of differentiating a canonical invariant, we must begin with a general change of axes, and after differentiation suppose that these are the canonical axes, and put $a_4 = b_4 = \&c., = 0$, as above. For this purpose, I show that if

$$X = \frac{\xi}{\omega}, \qquad Y = \frac{\eta}{\omega}, \qquad Z = \frac{\zeta}{\omega},$$

denote any homographic transformation, we shall have

$$A_n = \left(\frac{\omega^2}{\omega\xi_1 - \omega_1\xi}\right)^{n-3} \Big[a_n + \phi_1 \frac{\omega_1}{\omega} + \psi_1 \frac{\omega\xi_2 - \omega_2\xi}{\omega\xi_1 - \omega_1\xi}$$
$$+ \phi_2 \frac{\omega_2}{\omega} + \psi_2 \frac{\omega\xi_3 - \omega_3\xi}{\omega\xi_1 - \omega_1\xi}$$
$$+ \&c. \quad + \quad \&c. \quad + R\Big],$$

where R contains squares and products of ω_1/ω, $\omega\xi_2-\omega_2\xi/\omega\zeta_1-\omega_1\xi$, &c., where ω_n and ξ_n have a significance similar to that of y_n and z_n. In the earlier part of the paper I have shown that, if $\xi = x+q_1y$ and $\omega = 1+px+qy$, where the coefficients are small,

$$\begin{aligned} A_n = a_n - q_1\{(n-3)\,y_1a_n + \mathrm{S}ma_my_{n-m+1}\} \\ +p\{2(n-3)\,xa_n+(n-2)\,a_{n-1}\} \\ +q\{x[(n-3)\,y_1a_n+\mathrm{S}ma_my_{n-m+1}]+(n-3)\,ya_n+(n-2)\,y_1a_{n-1} \\ +\mathrm{S}\,(m-1)\,a_my_{n-m}\},\end{aligned}$$

and therefore we can determine the functions ϕ and ψ and get

$$\begin{aligned} A_n = \left(\frac{\omega^2}{\omega\xi_1-\omega_1\xi}\right)^{n-3}\Big[a_n &+ \left\{(n-2)\frac{\omega_1}{\omega}-(n-1)\frac{\omega\xi_2-\omega_2\xi}{\omega\xi_1-\omega_1\xi}\right\}a_{n-1} \\ &+\left\{(n-3)\frac{\omega_1}{\omega}-(n-2)\frac{\omega\xi_3-\omega_3\xi}{\omega\xi_1-\omega_1\xi}\right\}a_{n-2} \\ &+\dots\dots \\ &+\left\{2\frac{\omega_{n-3}}{\omega}-3\frac{\omega\xi_{n-2}-\omega_{n-2}\xi}{\omega\xi_1-\omega_1\xi}\right\}+\mathrm{R}\Big],\end{aligned}$$

and similarly

$$\begin{aligned} B_n = \left(\frac{\omega^2}{\omega\xi_1-\omega_1\xi}\right)^{n-2}\Big[b_n &- \left\{\frac{\omega_1}{\omega}-2\frac{\omega\xi_2-\omega_2\xi}{\omega\xi_1-\omega_1\xi}\right\}a_n \\ &+\left\{(n-2)\frac{\omega_1}{\omega}-(n-1)\frac{\omega\xi_2-\omega_2\xi}{\omega\xi_1-\omega_1\xi}\right\}b_{n-1} \\ &+\dots\dots \\ &+\left\{\frac{\omega_{n-2}}{\omega}-2\frac{\omega\xi_{n-1}-\omega_{n-1}\xi}{\omega\xi_1-\omega_1\xi}\right\}+\mathrm{R}\Big].\end{aligned}$$

If we now choose the new axes to be the canonical axes, we shall have, at the origin

$$\frac{\omega_1}{\omega} = -\left(2u_4+3\frac{Q_6}{P_6}\right) = -\mathrm{I},$$

$$\frac{\omega_2}{\omega} = -\left(3u_5-a_5+3b_4-3u_4\frac{Q_6}{P_6}-3\frac{Q_6^2}{P_6^2}\right) = -\mathrm{J},$$

$$\frac{\omega_3}{\omega} = -\left(v_6-3u_5\frac{Q_6}{P_6}+\frac{Q_6^3}{P_6^3}\right) = -\mathrm{K},$$

$$\dots\dots = \dots\dots$$

$$\frac{\omega_n}{\omega} = -\mathrm{J}b_n-\mathrm{K}a_n,$$

$$\frac{\omega\xi_2-\omega_2\xi}{\omega\xi_1-\omega_1\xi}=-\left(u_4+2\frac{Q_6}{P_6}\right)=-S,$$

$$\frac{\omega\xi_2-\omega_2\xi}{\omega\xi_1-\omega_1\xi}=-\left(u_5-\frac{Q_6^2}{P_6^2}\right)=-T,$$

$$\frac{\omega\xi_n-\omega_n\xi}{\omega\xi_1-\omega_1\xi}=-Sb_n-Ta_n.$$

We can now write down the leading terms in the expansions of α_n and β_n, and we can differentiate the expressions, remembering that after differentiation we put $a_4=b_4=\ldots.=0$ $a_6=\alpha_6$, $a_7=\alpha_7$, &c.

All the expressions I, J, K, &c., vanish if not differentiated, and all the part contained in R vanishes after differentiation as well. We have then only to consider that part of the differential coefficient of I, J, K, &c., which does not vanish. Writing this [I′], &c., we get

$$[I']=21\beta_7/\alpha_6, \qquad [S']=14\beta_7/\alpha_6,$$
$$[J']=12\alpha_6, \qquad [T']=6\alpha_6,$$
$$[K']=0,$$

and if $[\alpha_n']$ stands for the invariantive part of the differential coefficient of α_n, being the only part when referred to the canonical axes, we have

$$\begin{aligned}[\alpha'_n]=(n+1)\,\alpha_{n+1}&-3\beta_n\\ &-12\alpha_6\{(n-3)\,\alpha_{n-2}+\ldots.+(n-m-1)\,\beta_m\alpha_{n-m}+\ldots.+2\beta_{n-3}\}\\ &+6\alpha_6\{(n-2)\,\alpha_{n-2}+\ldots.+(n-m)\,\alpha_{m+1}\alpha_{n-m}+\ldots.+3\alpha_{n-2}\}\\ &-7\frac{\beta_7}{\alpha_6}\Big\{(n-4)\,\alpha_{n-1}-\ldots.-2(n-m-1)\,\beta_{m+1}\alpha_{n-m}-\ldots.\\ &\qquad\qquad -6\beta_{n-2}\Big\}\end{aligned}$$

and

$$\begin{aligned}[\beta'_n]=(n+1)\,\beta_{n+1}&-7\frac{\beta_7\alpha_n}{\alpha_6}\\ &-12\alpha_6\{(n-3)\,\beta_{n-2}+\ldots.+(n-m-1)\,\beta_m\beta_{n-m}+\ldots.+\beta_{n-2}\}\\ &+6\alpha_6\{(n-2)\,\beta_{n-2}+\ldots.+(n-m)\,\alpha_{m+1}\beta_{n-m}+\ldots.+2\alpha_{n-1}\}\\ &-7\frac{\beta_7}{\alpha_6}\{(n-4)\,\beta_{n-1}-\ldots.-2(n-m-1)\,\beta_{m+1}\beta_{n-m}-\ldots.\\ &\qquad\qquad -4\beta_{n-1}\}.\end{aligned}$$

This shows the mode by which the general values of these differential coefficients are found. The mode by which the series of canonical invariants can be consecutively determined follows, and the relations connecting the canonical invariants of a curve with those of

a reciprocal curve are then obtained, but the line of proof cannot be included in this abstract.

In the latter part of the paper, the differential equations of the quadriquadric curve are integrated to obtain the absolute invariant of the curve, and the conditions to be satisfied by the four excubo-quartics having closest contact with a curve at a point are found.

The equations to a quadriquadric curve referred to the canonical axes at a point of the curve, are

$$\left.\begin{aligned} u &= y-x^2-p_3(xz-y^2) = 0, \\ v &= z-xy-p_1z^2-p_2(xz-y^2) = 0, \end{aligned}\right\}.$$

where $p_1 = \alpha_6$, $p_2 = \alpha_7/\alpha_6$, $p_3 = \beta_7/\alpha_6$.

These represent two out of the family of quadrics which contain the quadriquadric; the quadric represented by $v = 0$ is that which touches at the origin the osculating plane to the quadriquadric at the origin. If we call the fourth point, at which that osculating plane meets the curve, the tangential of the origin, the quadric represented by $u = 0$ is that quadric of the family which touches, at the tangential, the osculating plane at the tangential.

These two quadrics are called the canonical quadrics at the point, and it is shown how to find the equations to the canonical quadrics at any point and how to express the canonical invariants at any point in terms of those at the origin. The relations between the invariants at a point and its tangential are found, and lead to the discussion of the singular points indicated by $p_1-p_2p_3 = 0$ and $p_1^2-p_1p_2p_3-p_3^3 = 0$.

XI. "On the Singular Solutions of Simultaneous Ordinary Differential Equations and the Theory of Congruencies." By A. C. Dixon, M.A., Fellow of Trinity College, Cambridge, Professor of Mathematics in Queen's College, Galway. Communicated by J. W. L. Glaisher, Sc.D., F.R.S. Received June 7, 1894.

(Abstract.)

§ 1. This paper is an attempt to shew how the singular solutions of simultaneous ordinary differential equations are to be found either from a complete primitive or from the differential equations.

The *latter* question has been treated by Mayer ('Math. Ann.,' vol. 22, p. 368) in a somewhat different way, but with the same result. He also gives a reference to a paper in Polish by Zajączkowski (summarised in vol. 9 of the 'Jahrbuch der Fortschritte der Mathematik'), and to one by Serret in vol. 18 of 'Liouville's Journal.'

The general result is that there may be as many forms of solution as there are variables (the differential equations being of the first order, to which they may always be reduced). Each form is derived from the one before by the process of finding the envelope, and each contains fewer arbitrary constants by one than the form from which it is directly derived.

The general theory is given in § 2 for the case when the differential coefficients are given explicitly in terms of the variables. In § 3 it is extended to the case when they are given implicitly, and in § 4 it is shown how the singular solutions are to be formed from the differential equations themselves. In §§ 5—9 the theory is connected with that of consecutive solutions belonging to the complete primitive. §§ 10—13 are taken up with geometrical interpretations relating to plane curves, and also to curves in space of $n+1$ dimensions, $n+1$ being the number of variables. In §§ 14—16 the case is discussed in which a system of singular solutions is included in a former system or in the complete primitive.

The rest of the paper contains the application of the theory to certain examples. The first example (§§ 17—21) is the case of the "lines in two osculating planes" of a twisted curve, and in particular of a twisted cubic. The particular example is given by Mayer and Serret. The second (§§ 22—26) is that of the congruency of common tangents to two quadric surfaces, and generally (§§ 27—38) of the bitangents to any surface. The third (§§ 39—49) is that of the essentially different kind of congruency which consists of the inflexional tangents to a surface. It seems natural to call these two kinds of congruency *bitangential* and *inflexional* respectively. The fourth example (§§ 50—52) is that of a system of conics touching six planes. The fifth (§§ 53—60) is that of a doubly infinite system of parabolas in one plane, the differential equation being a case of an extension of Clairaut's form $y = px + f(p)$, which is explained in §§ 53—55.

XII. "The Spectrum Changes in β Lyræ. Preliminary Note." By J. Norman Lockyer, C.B., F.R.S. Received June 13, 1894.

The spectrum of this well known variable star was first investigated photographically by Professor Pickering, at Harvard College Observatory, and a preliminary account of the results was published in 1891.* Dark and bright lines were found to be associated in the spectrum, and further, the bright lines were found to change their positions with respect to the corresponding dark ones according to the interval of time which had elapsed since the preceding minimum.

* 'Ast. Nach.,' 2707; 'Observatory,' 1891, p. 341.

It may be remarked that the period of the light-changes of the star is about twelve days twenty-two hours, and there are two approximately equal maxima of mag. 3·4, a principal minimum of mag. 4·5, and a secondary minimum of 3·9, the period of variation stated being that which elapses between two successive principal minima.

Professor Pickering found that during the first half of the period—that is, between principal and secondary minima—the bright lines were on the less refrangible sides of the corresponding dark ones, while during the second half they were displaced to the more refrangible sides. He further remarked that "the actual changes in the spectra, when studied in detail, are much more complicated than has been stated above, and show a variety of intermediate phases and changes in the dark as well as in the bright lines."

At Professor Pickering's request, I took up the work at Kensington in July, 1891, the instrument employed being the 6-inch Henry object glass and prism of 7½°, which I have described in a previous communication.* Several photographs were taken with this instrument, but it was not until the new 6-inch prism of 45°† was employed in the research that any considerable advance was made. With the higher dispersion of this instrument the spectrum is depicted in greater detail, and more minute changes can therefore be determined.

Since my work was commenced, accounts of the photographic spectrum of β Lyræ have been published by Belopolsky,‡ Father Sidgreaves,§ and Vogel,‖ and various suggestions have been made by them and others as to the conditions which bring about the variability.

On this account, although the reductions of the sixty-four photographs which I have obtained are not yet completed, I have thought it desirable to give a brief *résumé* of the facts already acquired.

For the complete study of the problem more photographs will be required, and a considerable amount of time will be required for the discussion of them. The present communication, therefore, is limited to a preliminary consideration of the variation in the spectrum as photographed at Kensington, and I have consequently in it omitted reference to the results obtained by other workers. In a subsequent paper, however, a complete history of the subject will be given.

To facilitate references to the spectrum, thirteen photographs—

* 'Phil. Trans.,' 1893 (A), vol. 184, p. 678.

† *Ibid.*, p. 679.

‡ 'Mem. Soc. Spett. Ital.,' June, 1893.

§ 'Monthly Notices, R.A.S.,' 1894, p. 96.

‖ 'Sitzungsberichte,' Berlin, February, 1894.

roughly one for each day of the period—are given in Plate 1. These have been enlarged about three times from the original negatives.*

As it is a matter of great difficulty to mount a series of such photographs showing the exact coincidences of the lines, in comparing the different spectra in the plates some allowance must be made for the slight differences in scale. Further, it is right to add that probably some of the fainter lines shown in the photographs are artificially produced by the process of enlargement, but the real lines will be readily identified by their appearance in more than one spectrum; the lines of particular interest are indicated in Plate 2 (p. 283).

The light curve which forms part of Plate 1 is constructed after Argelander's drawing,† and the dotted lines drawn from the spectra to the period scale indicate the relation of each photograph to the light curve.

I proceed to state, step by step, the results of the preliminary examination of the photographs, and to indicate the spectral phenomena on which they are based.

1. *The spectrum is constant at the same interval from principal minimum.*

Apart from the slight differences which seem to be accounted for by differences in the atmospheric conditions and consequently in the quality of the negatives, the spectrum appears to be the same at the same interval from minimum. The photographs reproduced in Plate 1 have been selected as being specially suitable for reproduction, but at most of the phases duplicates which are practically identical have been obtained.

2. *The kinds of variation shown on the photographs are as follows:*—

(*a.*) Periodical changes in the relative intensities of the lines.
(*b.*) Periodical doublings of some of the dark lines.
(*c.*) Periodical changes in the positions of the bright lines with respect to the dark ones.

3. *There are two bodies involved giving dark line spectra.*

On reference to Plate 1 it will be seen that at, and just before and after the second maximum, some of the dark lines are doubled. This indicates two sources of light giving dark line spectra and moving relatively to each other in the direction of the line of sight. When the relative movement in the line of sight is zero, none of the lines are doubled. The latter condition occurs about the time of the two minima.

4. *The maximum relative velocity of the two dark line components in the line of sight is about* 156 *miles per second.*

* This plate is not given in the "Proceedings," as it is very difficult to reproduce it on so small a scale.

† De Stella β Lyræ Disquisitio.

The greatest separation of the dark lines occurs about the time of second maximum, and the relative velocity, as determined by measurements of three of the doubles in the photograph of August 24, 1893, is that stated above. The individual measurements are as follows:—

$$H\gamma = 155 \text{ miles per second.}$$

$$H\delta = 154 \quad ,,$$

$$\lambda\, 4025 = 158 \quad ,,$$

5. *One of the dark line components bears a strong resemblance to Rigel and the other to Bellatrix.*

The spectra of the two components can readily be separated, for the reason that only lines common to both will be doubled. Among these are the lines of hydrogen. Lines special to either component are always single, and they retain the same relative positions with respect to one group of hydrogen lines throughout the period.

In Plate 2 photographs are given to facilitate an analysis of the compound dark line spectrum. At the bottom of the diagram is a reproduction of a photograph taken near the time of second maximum (August 24, 1893), and the spectra of Rigel and Bellatrix are included in the same plate. The compound character of the dark line spectrum of β Lyræ at this time is shown by the fact that one group of lines corresponds very closely with those which appear in the spectrum of Rigel, and when these are subtracted from the whole spectrum, a spectrum closely resembling that of Bellatrix remains; the latter spectrum being displaced in this photograph to the more refrangible side, as shown by the short lines drawn beneath the spectrum. The resemblance of the two components to Rigel and Bellatrix respectively, the spectra of which I have described in a previous paper,* is further shown by the following tabular comparison, the two dark line components of β Lyræ being called R and B respectively (p. 282).

It is not intended to suggest that the spectra of the two dark line components are quite identical with those of Rigel and Bellatrix. These are simply the best known stars which they most closely resemble, and the similarity is pointed out as an indication that we have not to deal with bodies of an unfamiliar type. Throughout the paper I shall refer to the two components as R and B respectively.

The conditions at first maximum, as shown in Plate 1, are not so simple as those at second maximum, though there is evidence to show that at this point of the light curve the component B is receding with respect to R. As will be seen on reference to the photograph of March 13, 1894, the hydrogen lines are broadened, and the

* 'Phil. Trans.,' 1893 (A), vol. 184, p. 693.

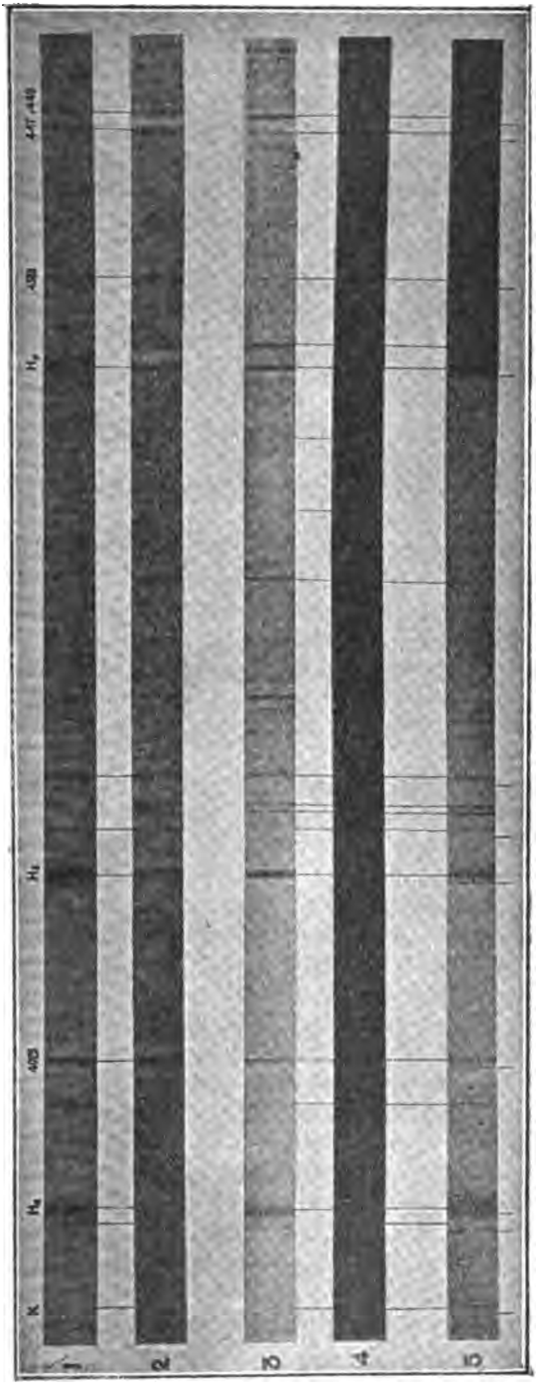
Component R.	Rigel.		Component B.	Bellatrix.	
Wave-length.	Wave-length.	Intensity.	Wave-length.	Wave-length.	Intensity.
				3919	2
				3926	3
3933	3933	6	3933	3933	3
	3963	2	..	3963	3
3968	3968	6	3968	3968	6
3994	3994	1	..	3994	3
4008	4008	2	..	4008	5
4025	4025	3	4025	4025	6
				4040	2
				4069	2
				4071	2
				4075	2
4101	4101	6	4101	4101	6
				4104	2
				4119	2
4120·5	4120·5	2	4120·5	4120·5	4
4127	4127	3			
4130	4130	3			
4143	4143	2	4143	4143	5
				4168	3
	4172	1	..	4172	1
	4177	1	..	4177	1
4233	4233	2			
				4241·5	2
				4253	2
4267	4267	2	..	4267	4
4340	4340	6	4340	4340	6
				4345	2
4351	4351	1	4351	4351	2
4388	4388	3	4388	4388	5
				4394·3	2
				4414·5	2
				4417	2
				4437	3
4471	4471	4	4471	4471	6
4481	4481	5	4481	4481	3
				4553	3

two lines near 4471 and 4481 have approached each other, as they should do if one belongs more especially to R and the other to B.

6. *When the two bodies lie along the line of sight, partial eclipses occur. This happens near the minima of the light curve.*

The differences in the intensities of the dark lines special to R and B, near the two minima, indicate that near the principal minimum R is partially eclipsed by B, while near secondary minimum B is partially eclipsed by R. These changes will be seen on Plate 1, and again in Plate 2. In the latter we have comparisons of β Lyræ at the two minima with Bellatrix and Rigel. If we leave the bright lines out of consideration, it will be seen that near principal minimum,

Fig. 1. (Plate 2.)



The photographs show that about the time of principal minimum, the dark line spectrum of β Lyræ (2) is very similar to that of Bellatrix (1), while about the time of secondary minimum the spectrum of β Lyræ (3) becomes more like that of Rigel (4), the differences at these times being mainly in the intensities of the lines. The photograph of the spectrum about the time of 2nd maximum (5) shows that there are two spectra displaced with respect to each other. The spectrum displaced to the less refrangible side is shown to resemble that of Rigel, while that displaced to the more refrangible side more closely resembles Bellatrix.

the spectrum of β Lyræ greatly resembles that of Bellatrix, the component B in this case lying between us and component R. As the eclipse is not total, however, the lines special to R appear with reduced intensities; the lines joining the spectrum of β Lyræ to that of Bellatrix indicate the principal lines of component B. At the secondary minimum, on the other hand, component R lies in front of component B, and the spectrum consequently bears a greater resemblance to that of Rigel. This is shown by the lines joining those of β Lyræ to the spectrum of Rigel in Plate 2.

The difference is especially noticeable in the case of the lines near λ 4471, 4481, 4388, and in the group of four lines a little less refrangible than H_δ. It will be seen that near principal minimum 4471 is stronger than 4481, as in Bellatrix, while about secondary minimum 4481 is stronger than 4471.

If the eclipses were total, the variations of the spectrum might be expected to be still more striking.

7. *In addition to dark lines there are several bright ones, which change their positions with respect to the dark ones.*

The photographs show conspicuous bright lines about wave-lengths 4862(H_β), 4715, 4471, 4388, 4340(H_γ), 4101(H_δ), 4025, and 3887(H_ζ). Other fainter ones also appear in some of the best photographs. The line at 4471 (Lorenzoni's f) is the well-known line which appears in the spectrum of the solar chromosphere, and those at 4025 and 4715 are amongst the brightest lines photographed with the prismatic camera during the total eclipse of the sun on April 16, 1893.

The displacements of the bright lines described by Pickering are confirmed in the main by the Kensington photographs. In the first seven photographs in Plate I taken between principal and secondary minimum, the bright lines lie on the less refrangible sides of the dark ones, at secondary minimum the broad bright lines are almost bisected by dark ones; while from secondary minimum to principal minimum the bright lines are more refrangible than the dark ones. The investigation of the movements of the bright lines must, however, be now carried on in the light of the knowledge gained with regard to the existence of two sets of dark lines.

If we consider the displacements of the bright lines with reference to the dark lines of component R, we find that they are always in the same direction as those of component B with respect to R. Thus in the first half of the period, the bright lines, as well as the dark lines of component B, are less refrangible than those of component R, while during the second half they are more refrangible. The bright lines, however, do not keep a constant position with respect to those of component B, although displaced in the same direction.

8. *The bright lines are brightest soon after secondary minimum.*

If the brightness of the lines in reality remains constant, they will appear relatively brightest at the two minima, owing to the reduction of continuous spectrum which is associated with the increased brightness of the star at maximum, and for the same reason they should appear brighter at principal than at secondary minimum. Estimates of the brightness of the lines in relation to the continuous spectrum have been made independently by four of my assistants, and, although estimates of this kind are liable to error, the general agreement is sufficient to indicate that when all allowance is made for the varying continuous spectrum, there is a maximum of brightness of the bright lines about half a day after secondary minimum. The apparent increase of brightness near principal minimum seems to be due solely to the reduced intensity of the continuous spectrum.

I have to express my obligations to Messrs. Fowler, Baxandall, Shackleton, Butler, Wardale, Crabtree, and North, who, at different times, have assisted in taking the photographs.

XIII. "On the Photographic Spectrum of the Great Nebula in Orion." By J. NORMAN LOCKYER, C.B., F.R.S. Received June 13, 1894.

(Abstract.)

The paper consists of a description and discussion of photographs of the spectrum of the Orion Nebula, taken with the 30-inch reflector at Westgate-on-Sea in February, 1890, of which a preliminary account was communicated to the Royal Society at the time. Fifty-four lines are tabulated as belonging to the spectrum of the nebula, nine of them being due to hydrogen. Tables are given showing:—

1. The wave-lengths, intensities, and probable origins of the lines photographed in the spectrum of the nebula.
2. A comparison of the lines in the spectrum of the nebula with lines in the spectra of (*a*) P. Cygni, (*b*) bright line stars and planetary nebulæ, and (*c*) stars in Groups II, III, and IV, of the classification according to the meteoritic hypothesis.

The complete discussion has led to the following general conclusions:—

1. The spectrum of the nebula of Orion is a compound one consisting of hydrogen lines, low temperature metallic lines and flutings, and high temperature lines. The mean temperature, however, is relatively low.*

* 'Roy. Soc. Proc.,' vol. 43, p. 152, 1887.

2. The spectrum is different in different parts of the nebula.

3. The spectrum bears a striking resemblance to that of the planetary nebulæ and bright line stars.

4. The suggestion, therefore, that these are bodies which must be closely associated in any valid scheme of classification, is confirmed.

5. Many of the lines which appear bright in the spectrum of the nebula appear dark in the spectra of stars of Groups II and III; and in the earlier stars of Group IV, and a gradual change from bright to dark lines has been found.

6. The view, therefore, that bright line stars occupy an intermediate position between nebulæ and stars of Groups II and III is greatly strengthened by these researches.

XIV. "On the Absorption Spectra of Dilute Solutions." By THOS. EWAN, B.Sc., Ph.D., 1851 Exhibition Scholar in Chemistry in the Owens College. Communicated by Professor H. DIXON, F.R.S. Received April 7, 1894.

(Abstract.)

The measurements recorded in the paper were made in the hope of obtaining some information as to the molecular condition of salts in dilute solution.

In order to obtain exact quantitative results it is necessary to measure the extinction coefficients of the solutions. For this purpose a new spectrophotometer was devised, by means of which it was possible to work with very dilute solutions. In this instrument, by the advice of Professor A. Schuster, F.R.S., a Lummer and Brodhun photometric prism was used, and the photometric measurements were made by means of Abney's rotating sector.

The absorption spectra of solutions of cupric sulphate, chloride bromide and nitrate, containing generally 0·003 to 0·004 gr. mol. per litre, were measured and found to be, within the limits of experimental error, identical. The solutions of cupric acetate absorb, for the same amount of copper, much more light than those of the other salts used. The difference tends to disappear as the solutions become more dilute, and it is increased by the addition of acetic acid. These facts point to the conclusion that the difference is due to the incomplete electrolytic dissociation of the salt, and to the undissociated part having an absorption spectrum differing from that of the dissociated part.

Dilute solutions of the potassium and ammonium salts of α-dinitrophenol (1·2·4) were found to possess very nearly the same absorp-

tion spectrum. The mean of the numbers obtained for these two salts was regarded as the absorption spectrum of the ion $C_6H_3(NO_2)_2O$. The solution of dinitrophenol in hydrochloric acid (containing the undissociated molecule $C_6H_3(NO_2)_2OH$) absorbs very little light; it is almost colourless. The extinction coefficients of dinitrophenol, and of its coloured ion, being thus known, it was possible to calculate from measurement of the extinction coefficients of a series of solutions of dinitrophenol in pure water, its degree of dissociation in these solutions. The numbers thus obtained were in very satisfactory agreement with the numbers calculated from the electrical conductivity of the solutions.

As an example of the hydrolytic decomposition of a salt in aqueous solution, to the study of which the spectro-photometric method can be advantageously applied, ferric chloride was taken.

By filtering dilute solutions of ferric chloride through a porous cell all the colloid ferric hydroxide formed can be removed; and analyses of the solutions before and after filtration showed that the hydroxide formed in solutions containing less than 0·005 gram molecule of $FeCl_3$ per litre contains no chlorine. The decomposition which occurs in these solutions may thus be most simply expressed by the equation $FeCl_3 + 3H_2O \rightleftarrows Fe(OH)_3 + 3HCl$.

The photometric determinations of the quantity of ferric hydroxide formed in these solutions agreed fairly well with the results of the filtration experiments, though, owing to the difficulty in obtaining the solutions of ferric chloride perfectly clear, they were not so satisfactory as could be desired.

The quantities of ferric hydroxide formed were not in agreement with the law of Guldberg and Waage, but agreed much better with the modified form of the law due to Arrhenius, in which the electrolytic dissociation of the different substances is taken into account.

It was observed that solutions of ferric hydroxide obtained by dissolving ferric chloride in a very large quantity of water, had a different absorption spectrum from that of solutions of ferric hydroxide obtained by dialysis. It is suggested that an explanation of this fact may be found in the differences in the complexity of the molecular aggregates existing in the different solutions.

Finally, solutions of ferric chloride, to which small quantities of hydrochloric acid had been added, possess such comparatively small power of absorbing light that they cannot be regarded as containing any colloid hydroxide of iron.

XV. "Researches on the Structure, Organisation, and Classification of the Fossil Reptilia. Part IX. Section 4. On the Gomphodontia." By H. G. SEELEY, F.R.S. Received June 21, 1894.

(Abstract.)

The Gomphodontia is a group of Anomodont reptiles characterised by theriodont dentition, in which the molar teeth are expanded transversely, more or less tuberculate, and have the crowns worn down with use, as in ungulate and other mammals. The orbit of the eye is distinct from the zygomatic vacuity, which is conditioned as in the Cynodontia, there being a long narrow parietal crest dividing the temporal vacuities. There are two well-defined occipital condyles united at the base, in a way that is closely paralleled in some mammals. The occipital plate is triangular, as in mammals, with no perforation except the foramen magnum. A deep superior notch defines the occipital plate from the lateral external squamosal bar. The malar bone, which forms the larger part of the zygoma, behind the orbit, has a slight descending process which varies in development. The hard palate terminates transversely in the middle length of the molar teeth. There is a descending transverse palatine arch situate behind the orbits. The incisor teeth are small and pointed; the canine teeth may be inconspicuous, but are usually large, compressed, and serrated; the premolars are small, circular, and usually tuberculate; the molars are usually single-rooted, in close-set series which diverge as they extend backward, with crowns which vary in form, but are commonly wider than long, and usually have the external and internal cusps more prominent than the other tubercles on the crown.

The group is based chiefly upon the genera *Gomphognathus*, known from skulls, a vertebra, and fragments of limb bones; *Trirachodon*, known from skulls only; and *Microgomphodon*, in which the canine teeth are no larger than the incisors. The last genus appears to make known the more important parts of the skeleton.

These specimens, collected by the author at Lady Frere, by Dr. Kannemeyer, near Burghersdorp, and by Mr. Alfred Brown, near Aliwal North, are all from the Upper Karroo rocks, on or about the horizon of the Coal Beds.

Of *Gomphognathus* there is a complete skull, with the lower jaw attached, about 9 ins. long, a second skull which displays the palate, and a separate lower jaw in connexion with part of the back of the skull. These specimens show four incisor teeth in each premaxillary bone, with sharp lateral serrated borders. The mandibular canine is covered when the jaws are closed. The maxillary canine is a

powerful tooth: its extremity is worn obliquely. There appear to be six premolar teeth, all contained in a length of half an inch. The maxillary teeth are packed in close succession, as in Rodents. There are nine molar teeth. In the middle, where they are largest, four occupy the length of 1 in. The contour of the crowns of these molars is convex from front to back, as in many mammals; and in this genus they are all behind the hard palate. The external cusp is prominent, and a ridge descends inward and backward from it upon the large flattened ledge of the crown, which is worn almost level, as though there were a rodent-like horizontal movement of the lower jaw.

A lumbar vertebra, found in developing the back of a skull, may possibly belong to this genus.

With the skulls a right humerus was found, which is $5\frac{1}{2}$ ins. long. It shows the reptilian transverse elongation of the proximal articulation, combined with characters which are paralleled in the marsupial mammals and Carnivora.

The genus *Microgomphodon* is known in the first place from a skull $2\frac{1}{2}$ ins. long, shaped much as in *Galesaurus*, but distinguished by the comparatively large size of the front pair of mandibular incisors, and the strong, conical, pointed character of the incisor teeth. The canine teeth are not differentiated from the incisors. The molars show in lateral aspect small blunt cones; but on their palatal aspect have flattened crowns with many small cusps. All the teeth have short roots. There are three incisors on each side in both the mandible and skull, one canine, and five molars.

There is ground for associating with this genus an imperfect skeleton, which, in addition to indicating ten early dorsal ribs, and fourteen lower dorsal vertebræ and ribs in advance of the acetabulum of the femur, shows the left humerus, portions of right and left scapulæ, portions of the coracoid, clavicle, interclavicle, the pelvic bones, all the bones of the hind limb, distal ends of ulna and radius, carpus, metacarpus, and five digits. With these a fragment of a skull is associated, which has the maxillary and mandibular teeth in contact, showing the animal to be Gomphodont; while so much as is preserved closely resembles the skull of *Microgomphodon*, and apparently the canine was not larger than the premolar. This skeleton demonstrates a close general resemblance of plan between the Gomphodontia and Cynodontia. The lower dorsal ribs have a transverse lozenge-shaped enlargement, which, however, is less developed than in *Cynognathus*. The pelvis is exposed on the ventral side. As in most, if not all, South African Therosuchia, it shows no indication of median division between the pubic bones, while the ischia retain their individuality.

The pubis articulated to a short tubercle on the ilium. The blade of

the ilium is thin, but imperfectly exposed; and the ischia are shaped as in Pliosaurus, but the pubis does not closely resemble that of any reptile. The femur has the inferior internal trochanteric ridge only slightly developed. There is no neck defining the head of the bone from the shaft. The fibula is slender; no indication of a patella is preserved. Below the stout tibia, the proximal row of the tarsus appears to consist of two bones, an inner astragalus with hemispherical proximal surface, and a narrow elongated bone which appears to be the calcaneum. There were three or four bones in the distal row of the tarsus, but only one is preserved. The digits are nearly parallel with each other, and the foot has a compact character like that of *Dicynodon.*

The scapulæ have the pre-scapula developed on the same plan as in *Cynognathus,* and the anterior margin of the bone reflected upward, so as to form the spine of the scapula, terminating in the acromion. The two ends of the humerus are twisted at an angle of 45 degrees, and the bone is expanded as in many Saurischian reptiles. The carpus shows three bones in the proximal row, a large reniform carpal below the ulna, regarded as the pisiform bone; a comparatively small middle carpal is identified as the cuneiform bone. The third bone corresponds with the scapho-lunar of *Theriodesmus;* it is beneath the radius. There is no indication of any pre-pollex. There are four bones in the distal row of the carpus. There are five digits.

In the pelvis and the limb bones this Anomodont type approximates to the Saurischia and Mammalia, just as the Ornithischia approximate to birds in the same parts of the skeleton.

Trirachodon is founded on four individuals which have the skull about 4 ins. long. Like the other Gomphodont genera, this type has the dentary bone developed so as to occupy the length of the mandible, but the lower jaw is composite, the internal bones filling the space which in mammals is occupied by the meckelian cartilage. The post-frontal and pre-frontal bones are well developed. The species differ in the character of the teeth, especially in number and form of the pre-molars.

In one species from Aliwal North, the molar teeth are transversely wide, ornamented with three transverse ridges, which terminate in a slight cusp, both on the external and internal margins. There are not more than nine molars. The crown of the first pre-molar in one specimen is elongated from front to back, and shows a small coronet of rounded marginal cusps. In a species from Lady Frere the molar teeth are narrower, and the pre-molar teeth more numerous, small, and circular in the broken sections.

Although these skulls are mammalian in aspect, and in some respects make new transitions towards mammals, in technical characters they retain a sufficient number of reptilian structures to

permit no doubt that they are true reptiles. The mammalian resemblances in the skull being paralleled in the other parts of the skeleton, it may be affirmed that these fossils demonstrate a closer affinity between reptiles and mammals than had previously been evident.

XVI. "Researches on the Structure, Organisation, and Classification of the Fossil Reptilia. Part IX. Section 5. On new Cynodontia." By H. G. Seeley, F.R.S. Received February 13, 1894.

(Abstract.)

The Cynodontia is a division of the Theriodontia in which there are long and large temporal vacuities in the skull, formed chiefly by the squamosal and malar bones; in which there is no descending pedicle to the squamosal bone; in which the occipital condyle is crescentic and imperfectly divided into two lateral parts; and in which the hinder molar teeth, larger than the incisor teeth, develop anterior and posterior cusps, are compressed from side to side, and overlap, with shearlike action, the teeth of the mandible. The principal new genera included in this group are *Cynognathus*, which is known from several skulls, and one fairly complete skeleton; and the genus *Tribolodon*, which does not differ in a striking way from the small Cynodonts previously known, referred to the genera *Galesaurus*, *Nythosaurus*, and *Thrinaxodon*.

The skeleton of *Cynognathus crateronotus* was found at Lady Frere, near Queenstown. A single tooth of this genus had already been obtained by Mr. Alfred Brown at Aliwal North. The skull is between 15 and 16 in. long, 8 in. high at the orbits, and higher at the occiput, where it was about 9 in. wide. The lateral aspect is remarkably Mammalian, owing to the great development of the dentary bone, which forms a new type of lower jaw, and has a greatly developed coronoid process, and the form of the zygoma. On the palate, the palatine and transverse bones form a descending arch between the rami of the mandible, as in Crocodiles, *Sphenodon* and Lizards. The composite structure of the lower jaw is seen on its inner side. The pre-frontal and post-frontal bones remain distinct. There is a small quadrate bone embedded in the large squamosal bone. The latter resembles that of Mammals, both in its extension along the zygoma, and its expansion as a squamous plate on the side of the brain case.

There are four incisors in each pre-maxillary; their margins are serrated. There appear to be but three mandibular incisors on each side, so that the type resembles *Cynochampsa*, but there is no evidence

of close affinity with that genus. The canine teeth are large, worn on the anterior border, and serrated on the hinder margin. Remnants of canine teeth are indicated which have been replaced by those which persist. There are nine molar teeth, of which the first five are smaller than the posterior teeth. Those teeth are more than half as wide again, from front to back, as the anterior teeth. The hinder teeth have the principal cusp directed backward, with one subordinate pointed cusp on the front margin, and two subordinate cusps on the hinder margin. The crowns of the teeth stand high above the alveolar margin in this species. They are intermediate in form of crown between *Canis* and *Zeuglodon.*

The nares are terminal, divided, lateral, and arch forward in front of the alveolar margin. The orbit of the eye is 8 in. behind the extremity of the snout, nearly circular, and separated from the temporal vacuity by the post-frontal bone. The post-frontal bones converge backward along the parietal crest. The malar bone develops a slight descending process on its inferior margin. There is no interorbital septum ossified. The type species of *Cynognathus* shows, on the one side preserved, a small post-orbital foramen, comparable to that of *Procolophon*, and the author considers that the enlargement of this foramen makes an essential difference in plan between the skulls of Teleosaurs and Theriodonts, and regards the Mammalian zygoma as resulting from the obliteration of the post-orbital vacuity which defines the superior and inferior temporal arcades in Saurischia and other Reptilia.

In general structure of palate *Cynognathus* resembles *Lycosaurus.* There is no transverse boundary to the hard palate, but the palatonares are lanceolate. The author finds that the downward development of the bones of the palate at the posterior borders of the nares, while thoroughly reptilian, approximates to the condition in Mammals.

The form of the lower jaw approximates to that of the older Mammals and lower Mammalian types, leading to the conclusion that the Mammalian lower jaw consists essentially of the dentary bone. The dentary bone is compared to that of *Microconodon* in form, and development of the angle of the jaw.

The shoulder girdle consists of a large scapula, small coracoid, and compressed pre-coracoid. The scapula demonstrates the origin of a spine like that of the scapula in Mammals, by outward development of the anterior border of the scapula in Reptiles. This spine is defined by a pre-scapula development anteriorly. The spine may have been originally a separate ossification, such as in *Pareiasaurus* has been named epi-clavicle. It terminates in an acromion which is reflected forward.

The humerus is imperfectly preserved, but has the distal con-

dyles well developed; and the proximal crest has a form which is seen in Marsupials, but the articular head is transverse.

The vertebral column measures 37 ins. from the body of the atlas to the last lumbar vertebra; and its total length is 45 ins., but the extremity of the tail is lost. There appear to be only six cervicals defined by the form and direction of the transverse processes for the tubercles of the ribs. The head of the rib is attached to the intercentral suture, and in the first vertebra reaches the intercentrum. There are 29 presacral vetebræ, of which 18 may be counted as dorsal and 5 as lumbar. The most distinctive feature of the vertebral column is the interlocking of the ribs in the lower dorsal and lumbar region, where the ribs become transversely expanded, and anchylosed to the side of the centrum. The neural arch in the lumbar region also interlocks, by an arrangement resembling the zygosphene and zygantrum of Serpents. No dorsal rib is completely preserved.

The sacrum is small; and the sacral ribs are smaller than the lumbar ribs. They are four in number. The middle two vertebræ are anchylosed. The caudal vertebræ are short, only four are preserved. They indicate a considerable movement. There is no evidence of dermal armour. The characters of the vertebral column described by Professor Cope, in *Dimetrodon* and allied genera, closely resemble *Cynognathus*.

The pelvis consists of three bones; the ilium forms an expanded plate more resembling *Megalosaurus* than *Dicynodon*. There is a large longitudinal obturator foramen, between the pubis and the ischium. The anterior transverse border of the pubis is cartilaginous, and there is no evidence of pre-pubic bones. The ischium is larger than the pubis. The author compares the anomodont pelvis with that of Plesiosauria, although *Pliosaurus*, in the form of the ilium, more closely approaches *Dicynodon* than *Cynognathus*.

The femur is imperfectly preserved. It was characterised, as in all Theriodonts known to the author, by the development of an immense inferior plate or ridge at the proximal end, which distinguishes it from allied animals. In this specimen the ridge is broken away. The head of the bone is greatly expanded transversely; and the distal end is not preserved.

Under the name *Cynognathus Berryi* the author describes imperfect evidence of a smaller skull of *Cynognathus*, which is distinguished from *C. craternotus* with some doubt; but if distinct it is defined by the relatively large size of the middle mandibular incisor, the apparent presence of ten molars, in all of which the crowns overlap each other, and the roots are barely shown at the alveolar border. In the small species the cutting margin and the cusps of the posterior teeth are better defined.

If the species are identical the teeth have probably yet to be re-

placed by a successional series, but no known specimen of any genus shows such replacement.

The skull of *Cynognathus platyceps* was obtained by Dr. Kannemeyr at Wonderboom. It is a small species distinct from *Cynognathus crateronotus.* The skull has lost the extremity of the snout. It is remarkable for its depression. The teeth, however, are similar to those of the larger species; they have five denticles. The composite structure of the lower jaw is well shown, and the dentary bone behind the angle of the jaw retreats so as to expose the elements which form the articulation.

The occipital plate of a large Theriodont skull from Lady Frere is described, which shows a circular foramen magnum, and the perfectly preserved occipital condyles which are not quite so completely separated as in Mammals, having only a median groove between them on the ventral surface.

Another fragment of a skull preserved in the Albany Museum has only the pre-orbital portion preserved, and is remarkable for the small size of its incisor teeth, widely separated from each other, and for having two canine teeth parallel to each other. On both sides the crowns are imperfectly preserved. The molar teeth are on the type of *Cynognathus,* with a principal cusp flanked back and front by a small cusp, with a smaller accessory posterior cusp in the four hindermost teeth. As in all species of the genus the mandibular symphysis is long, oblique, and completely obliterated. There is a large pit with sharp margin, in the median line in front of the orbits, which may be a generic difference from *Cynognathus,* since it occurs in the area in which other specimens show indications of a thin supranasal ossification flanked by a pair of small hemispherical concavities. It is indicated as *C. leptorhinus.*

Tribolodon Frerensis is the name given to a dentary bone with few three-pronged teeth widely separated from each other standing high above the jaw. With this jaw is associated a femur which shows the transverse development of the great trochanter as strongly developed at the proximal end of the bone as in *Ichthyosaurus,* so that the trochanter minor of Mammals only represents that of Theriodonts in miniature, the trochanter being more developed than in Saurischia or any other reptiles. With it is associated a right tibia which is somewhat curved and nearly as long as the femur.

These Cynodont remains have given no certain evidence of the extremities of the limbs; but with this exception they make known the entire skeleton for the first time in an African Theriodont, furnishing data for comparison with Mammals and Reptiles in every part of the skeleton preserved.

XVII. "Researches on the Structure, Organisation, and Classification of the Fossil Reptilia. Part IX. Section 6. Associated Remains of two small Specimens from Klipfontein, Fraserburg." By H. G. SEELEY, F.R.S. Received June 21, 1894.

(Abstract.)

The author obtained parts of two skeletons from the summit of the Karroo rocks, which form the Nieuwveldt range. They resemble Theriodonts in their general marsupial characters. The fragments of skulls are not in the same slabs with the other bones.

Theromus leptonotus shows the fore-limb and some vertebræ. The humerus is determined to be Theriodont by the transverse extension of the proximal articulation. The bone is $1\frac{4}{10}$ inches long, resembling in form that of the Phalangers. The ent-epicondylar foramen is more vertical than in the marsupials; and, as among marsupials, the radial crest if prolonged distally would be continuous with the bridge over that foramen. The vertebræ are each $\frac{3}{10}$ inch long; they show a transverse suture between the neural arch and the centrum.

The anterior part of the skull, very imperfectly preserved, indicates three incisor teeth with the root of a relatively large maxillary canine, but the region of the molar teeth is lost. There is also a posterior fragment of a skull, which makes known the bones of the palate and the base of the brain case seen from above. Enough is shown to indicate Theriodont characters, but the animal appears to diverge from the Theriodonts towards the Dicynodont type. If the base of the skull belongs to the same individual as the snout, it indicates a head nearly $4\frac{1}{2}$ inches long.

The second specimen shows 14 dorsal vertebræ, which occupy a length of $5\frac{1}{4}$ inches; each slightly exceeds $\frac{3}{10}$ inch in length, so that this animal named *Herpetocheirus brachycnemus*, is similar in size to the fossil previously described.

The centrum is deeply biconcave. There is no indication of a capitular articulation for the ribs. The ribs are slender, and the longest are $2\frac{1}{2}$ inches in length. There is no trace of the transverse expansion seen in *Cynognathus*, although the ribs preserved indicate 20 dorsal vertebræ. The humerus is $1\frac{6}{10}$ inches long, and is exposed on the superior aspect. It is distinguished from the type already described by wanting the tuberosity on its inner distal border, which has a convexly rounded contour. The radius is stronger than the ulna, but there is no indication of an olecranon process exposed. The ulna is no stouter than a rib. These bones are an inch long. The carpus shows one large bone below the radius; there is a smaller

bone on its outer side, which corresponds to the distal end of the ulna, but there is no trace of a third bone preserved, and there is only one central bone preserved. There are three phalanges in a digit. The femur is $1\frac{9}{10}$ inches long; its articular head appears to be small and hemispherical. There is a large internal trochanter extending down the shaft, which corresponds with the similarly placed ridge in the femur of Megalosaurs and other Saurischia.

The slender character of the ribs, which are different from those in known Theriodonts, suggests the possibility that these remains belong to a group distinct from both the Cynodontia and Gomphodontia.

A small badly preserved fragment of a skull found near to this fossil is described, but there appears to be no sufficient evidence for associating it with the other remains.

XVIII. "On the Evolution of the Vertebral Column of Fishes." By H. Gadow, Ph.D., F.R.S., and Miss E. C. Abbott. Received June 20, 1894.

(Abstract.)

Concerning the segmental mesodermal products the following subdivision is adhered to:—

The term *myotome* is to be restricted to the whole rest of the protovertebra after the skeletogenous cells have been given off for the production of the *sklerotomes*.

The sum total of the sklerotomes makes up the skeletogenous layer.

The ending *tome* to indicate the primary, or earlier, less differentiated; the ending *mere* to signify the final condition or product.

Consequently, the protovertebræ divide into—I, Myotomes, each of which produces (1) one myomere or segment of the general mass of trunk-muscles, (2) cutis; II, Sklerotomes which produce skleromeres or skeletal trunk segments.

Each protovertebra produces a dorsal and a ventral sklerotome; strictly speaking, one sklerotome which consists of a separate dorsal and ventral half.

The protovertebral segments are not transverse "plates," but are curved into S-shape, the top end curving tail- and inwards, the middle and ventral thirds bulging headwards, the amount of curvature being (in 7 mm. embryos of Acanthias) so great that a transverse plane will cut through the dorsal and ventral third of one, and through the middle portion of the next following segment.

This S-shaped curving and consequent overlapping of the protovertebral "plates" SS is of fundamental importance for our under-

standing of the formation of the vertebral column, because it explains (1) the so-called new segmentation of the axial column, (2) the almost universal occurrence of more than one dorsal and one ventral pair of arcualia (namely, arches and intercalary pieces) in each of the later vertebral segments or skleromeres.

The explanation is as follows:—

1. The dorsal half of sklerotome 2 grows downwards and comes to lie behind the ventral half of sklerotome 1.

2. The ventral half of sklerotome 2 grows upwards and comes to lie in front of and below the dorsal half of sklerotome 3.

3. The formation of a physiological unit is effected by the combination or fusion of the unequally numbered sklerotomic halves, in such way that the dorsal half lies behind and above the ventral half.

The new skleromere I (= dorsal sklerotome 2+ventral sklerotome 1) stands now in the following relation to the myomeres; the dorsal end of the skleromere I coincides with myomere I; the septum between this myomere and the next previous one passes between dorsal sklerotome 2 and ventral sklerotome 1; this means to say right across the new skleromere I. This skleromere lies within the influence or range of action of two successive myomeres. Taken as a whole, the skleromere is "interprotovertebral," more correctly biprotovertebral, because it is composed of two successive sklerotomes, namely, the ventral half of one and the dorsal half of a second.

Consequently, the "resegmentation" or "neugliederung" is brought about in a manner fundamentally different to that hitherto supposed to have taken place. If A and B mean two successive sklerotomes, a and b their dorsal, α and β their respective ventral halves, then the new skleromere is composed of $b+\alpha$ and not of $\frac{A+B}{2}$, because $b+\alpha$ is the same as $\frac{\text{B dorsal}}{2}+\frac{\text{A ventral}}{2}$.

The formation of a skleromere by the combination of alternating dorsal and ventral halves of sklerotomes explains also the presence of eight (four pairs) cartilaginous pieces, namely, basalia (so-called dorsal and ventral arches) and interbasalia (so-called intercalary pieces) for each complete segment.

The dorsal and ventral halves of the sklerotomes are pyramidal in shape, with their apices pointing respectively downwards and upwards. Each ventral pyramid extends with its apex above the chorda, and founds there (separated from the ventral mass by the subsequent rapid growth of the chorda and its sheath) a cluster of cells which remains henceforth behind (tailwards from) the basal mass of the dorsal pyramid. The latter founds, with its downgrowing apex, a colony of cells below the chorda, and in front of the basal ventral mass. Thus are produced the basalia and interbasalia,

each colony or cluster of cells developing into a separate piece of cartilage. The basidorsal does not fuse with its interdorsal, because both are the offspring of two different sklerotomes, nor can the basidorsal fuse with its own offspring, namely, with the interventral, because both became, and remain, separated by the chorda and its sheath; they are connected only by the indifferent connective tissue of the membrana reuniens, but not by cartilage-forming cells.

Concerning the formation of centra or bodies of the vertebræ, we distinguish:—

I. *Chorda-centra*, *i.e.*, centra cut out of the full of the chordal sheath, which itself has been strengthened by invasion of cartilaginous cells from the skeletogenous layer. This migration of cartilage into the chordal sheath had already been hinted at by Kœlliker more than thirty years ago; it has recently been proved by Klaatsch, and has been corroborated by us. Chorda-centra are possessed by all Elasmobranchs, potentially by Dipnoi and Holocephali.

II. *Arch-centra*, *i.e.*, centra formed by the skeletogenous mass which remains entirely on the outside of the chordal sheath, which latter takes no share in their formation: osseous Ganoids and Teleostei.

Chorda-centra and arch-centra represent two different modes of development, each starting from an acentrous condition. This can be expressed as follows:—

Chordal sheath remaining entirely chordagenous.	Chordal sheath strengthened by invasion of skeletogenous cells, therefore with possibility of chorda-centra.
Cyclostomata, Cartilaginous Ganoids.	Dipnoi and Holocephali.
Formation of Centra.	
Osseous Ganoids, Teleostei.	Elasmobranchs.
ARCH-CENTRA.	CHORDA-CENTRA.

The formation of chorda-centra being independent of the arcualia explains how and why the number of "centra" does not necessarily agree either with that of the arcualia or with that of the trunk-segments, *e.g.*, Hexanchus and tail of most other Elasmobranchs.

These leading differences and their modifications have been traced in Petromyzon, Acipenser, Amia, Lepidosteus, Protopterus, Chimæra, and in numerous Elasmobranchs.

In *Amia calva*, of which the adult and a young specimen of 57 mm. were examined, the *postcentrum*, *i.e.*, the posterior, archless disk of a complete tail-vertebra, was found to be formed by the interdorsalia and interventralia of the same sklerotome, while the *precentrum*, *i.e.* the arch-bearing disk or anterior half is formed by the basidorsals of

the same sklerotome and the basiventrals of the next previous sklerotome. Thus skleromere 50 is composed of a postcentrum = interdorsal 50 + interventral 50, and of a precentrum = basidorsal 50 + basiventral 49. The intermuscular septum runs obliquely across the precentrum, or, in other words, the precentra are bi-protovertebral or bi-myomeric, but not the postcentra. The precentra of the tail of Amia are homologous with the "pleurocentra" in the tail of the Jurassic Eurycormus, while Amia's postcentra are the same as the "hypocentra" of Eurycormus.

In *Lepidosteus osseus*, of which adult specimens and larvæ of various stages were examined, the combination of parts into one vertebral complex is superior to that of Amia, because each vertebra belongs, with its entire anterior half (interdorsal 50 + basiventral 50), to myomere 50, and with its posterior half (basidorsal 51 + interventral 51), to myomere 51. In other words, the vertebral mass is equally divided between two successive myomeres, or the myomeres have an equal share of the skleromeres. The vertebræ are now truly bi-protovertebral or bi-myomeric, each vertebra being composed of $a + b$.

XIX. "On the Structure and Affinities of *Heliopora cærulea*, Pall., with some Observations on the Structure of *Xenia* and *Heteroxenia*." By Gilbert C. Bourne, M.A., F.L.S., Fellow of New College, Oxford. Communicated by Professor Lankester, F.R.S. Received May 30, 1894.

(Abstract.)

I have had the opportunity of making a renewed examination of the structure of Heliopora, partly through the kindness of Professor Ray Lankester, who gave me a very well preserved fragment of a colony brought by Dr. S. J. Hickson from Talisse, Celebes. I have also used some specimens which I collected and preserved in spirit in Diego Garcia, and, in studying the dried corallum, I have had the advantage of a large collection, originally the property of the late Mr. George Brook, which Mrs. Brook has very kindly handed over to me.

All the specimens in my possession are referable to the only recent species known, *Heliopora cærulea*, but one of them belongs to the variety *tuberosa*, Dana.

The Soft Tissues.—These form an even sheet, investing the surface of the colony, interrupted here and there by the mouths of the polyps, which are the only apertures opening on the surface. The structures described below are entirely superficial, and there is no direct com-

munication between the polyps and their connecting canals on one side of the colony and those on the other side of the colony.

The polyps have been fully described by Moseley. They are scattered irregularly over the surface, and the only important feature presented by them, in which they differ from other Alcyonaria, is the complete introversion of the tentacles during retraction. Surrounding each polyp, and occupying all the surface of the colony, are very numerous tubes, ending blindly below, and closed above by the sheet of superficial ectoderm which covers the exterior surface; these are the *cœnenchymal cœca.* They occupy cavities in the corallum known as cœnenchymal tubes, and are set at right angles to the surface of the colony. The cœnenchymal cœca communicate with one another, and with the polyps, by means of a network of canals, which lies close beneath the surface; these are the *superficial endodermic canals.*

At the growing edges of the colony the superficial network is not well developed, the cœnenchymal cœca are closely contiguous, and open into one another at their outer ends, either directly or by means of short, irregular, transverse passages which cross over the partitions separating adjacent cœnenchymal tubes.

The cœnenchymal cœca, superficial canals, and polyps are lined internally with endoderm. Outside this is a thin layer of mesoglœa, and outside of this an irregular layer of large, dark-staining, granular, fusiform cells, which are calcigenous, and may be called *calicoblasts.*

The calicoblasts were described by Moseley as mesodermic, but they occupy the position of ectoderm, and they are, in fact, derived directly from the superficial sheet of ectoderm. Their origin is seen in sections made perpendicularly to the surface of the colony at its growing point. Here the ectoderm cells are generally elongate and pyriform, their broader outer ends resting on a distinct external limiting membrane, their inner ends tapering and produced into long processes, which may often be traced into connexion with deeper seated fusiform cells, of more granular character. The deeper seated cells are imbedded in a thick, homogeneous, gelatinous substance which lies immediately below the ectoderm, and is the newly formed mesoglœa, thicker here than elsewhere in the colony. Study of numerous sections shows that the deeper seated fusiform cells are derivatives of the elongate ectoderm cells, and that some of them are used up in the formation of the mesoglœa—they appear to dissolve and to be wholly converted into a structureless gelatinous mass—whilst others increase in size, develop many refracting granules in their interior, and become calcigenous calicoblasts. In many places the calicoblasts may be traced into direct connexion with the ectoderm. The cœnenchymal cœca of Heliopora do not appear to be degenerate siphonozooids, as was suggested by Moseley, but rather to

be specialised parts of a system of inosculating endodermic canals, such as are characteristic of colonial Alcyonaria.

The corallum of Heliopora exhibits two sets of apertures, besides those due to the inroads of boring parasites, these are the calicles and the *cœnenchymal fenestræ.* The calicle cavities are occupied by the polyps, the cœnenchymal tubes, whose mouths are the fenestræ, are occupied by the cœca, which do not in the fresh condition open to the surface, the fenestræ being closed above by the ectoderm.

The corallum consists of an imaginary vertical plane, occupied by vertically disposed cœnenchymal tubes, and right and left faces on which the tubes open after bending sharply from the vertical to take a short horizontal course. The vertical tubes are in section polygonal, and some of them attain the surface at the growing edge. Those which are deflected horizontally become thickened by the formation of secondary calcareous deposits inside the primitively polygonal tubes. The calicles are formed by the arrest in growth of groups of cœnenchymal tubes as they approach the surface. The cavity of a calicle never extends into the central vertical tubes.

The walls of each cœnenchymal tube are primarily formed of twelve delicate calcareous laminæ, secreted by the calicoblasts covering the cœnenchymal cœca, and have this peculiarity, that each of the laminæ takes a share in the formation of the walls of adjacent tubes. As seen in section, three laminæ are united at each angle of the generally hexagonal tube to form a Y-shaped figure. Each arm of the Y meets, and is united by sutures with the arms of adjacent Y's, and so a sort of honeycomb structure is produced, which, if the symmetry of growth were perfectly regular, would consist of a series of regular hexagons. The symmetry is disturbed by the multiplication of the tubes, which do not branch dichotomously, as in the allied Heliolites, but increase by the addition and intercalation of new tubes amongst those previously existing. The hexagonal primary constituents of the corallum of Heliopora show very slight traces of blue colour, but as they become thickened by secondary ring-shaped deposits, the latter develop blue pigment, and give the characteristic colour to the colony.

The growth of the colony is not effected, as Moseley described, by the upgrowth of an axial polyp from which lateral buds are given off, but by the rapid growth and multiplication of cœnenchymal tubes.

The fact that the calicles and cœnenchymal tubes of Heliopora have not each their distinct and proper wall, but that their walls are common to them and to adjacent tubes, is a characteristic feature of Heliopora and its allies. I therefore propose for them the name *Cœnothecalia*, in contradistinction to those forms in which, as in *Tubipora*, each corallite is separate and distinct; the latter group may be called the *Autothecalia.*

Under the *Autothecalia* I class *Tubipora*, *Syringopora*, *Syringolites*. the *Favositidæ*, and, provisionally, the *Columnariadæ*.

Under the *Cœnothecalia* I class *Heliopora*, *Heliolites*, *Thecia*, *Plasmopora*, *Propora*, *Lyellia*, the *Chætetidæ*, and, provisionally, *Tetradium*, *Halysites*, and the *Monticuliporidæ*.

The genus Heliopora is not the only Alcyonarian with a distinct ectodermic skeleton. I brought back with me from Diego Garcia two small Alcyonarians of the genus *Xenia*. One of the species is referable to *Xenia umbellata*, Savigny, var. *cærulea*. The other I am describing elsewhere as a new species, under the name *Xenia garciæ*. These forms both possess a discontinuous skeleton, formed of the minute corpuscle-like spicules characteristic of the Xeniidæ. In *X. umbellata*, the spicules in the exsert moieties of the polyps are wholly ectodermic, and none are found in the mesoglœa. In the stem the external ectoderm is filled with spicules, and the so-called cœnenchyme proves to be nothing more than the fused ectoderm of the basal moieties of the polyps, which is traversed by strands of mesoglœa binding the polyps together, and by endodermic canals which place the polyp cavities in communication with one another. The mesoglœa of the basal moieties of the polyps, as well as the connecting strands of mesoglœa, are quite free from spicules, which are, however, abundant in the mass of fused ectoderm occupying the spaces between the polyps.

In *Xenia garciæ* the spicules are, as in *X. umbellata*, ectodermic in the exsert moieties of the polyps, and in the ectoderm covering the stem. The basal moieties of the polyps are provided with a much thicker mesoglœa, which is, however, free from spicules, except where the mesoglœal laminæ of adjacent polyps become fused, in which case intrusive ectoderm cells and spicules are found in the fused thickened areas. Elsewhere the basal portions of the polyps are separated, as in *X. umbellata*, by ectoderm containing spicules, the mass of which is much less abundant than in *X. umbellata*. There is also in *X. garciæ* a special system of superficial endodermic canals, which lies immediately below the surface in the upper part of the stem.

I have further been able to examine some specimens of *Heteroxenia elizabethæ*, collected by the late Dr. Gulliver, at Zanzibar, and given by him to the Linacre Department at Oxford. I am able to confirm Kölliker's account of this genus, which exhibits a well-marked dimorphism, the colony consisting of fertile autozooids surrounded by more numerous sterile siphonozooids. The spicules of *Heteroxenia elizabethæ* resemble those of *Xenia umbellata* and *garciæ*, in being minute and entirely ectodermic in the exsert moieties of the polyps. The stem, however, differs considerably from that of *X. umbellata*, and is more specialised than that of *X. garciæ*. Instead of the

mesoglœal laminæ of the basal moieties of the polyps being distinct or only partially fused together, they are absolutely and indistinguishably fused, and the mesoglœa is enormously thickened, forming a cœnenchymal mass resembling that of Alcyonium. The mesoglœa immediately surrounding the polyp cavities is devoid of cells, but elsewhere it contains numerous intrusive cells, among which spicules are developed. The intrusive cells are derivatives of the ectoderm, and in suitable preparations numerous strands of cells are seen to pass inwards from the ectoderm, between the ramifications of the superficial set of endodermic canals, which is rather more marked in this species than in *X. garciæ*. It seems probable that the greater part of the cœnenchymal mesoglœa is formed at the expense of the intrusive ectoderm cells, very few of which develop spicules.

These three species are interesting, firstly, as indicating the steps by which forms with a wholly mesoglœal spicular skeleton, such as Alcyonium, may have been derived from forms with an ectodermic skeleton; and, secondly, as suggesting the mode in which the ectodermic skeleton of Heliopora may have been developed. In the Xeniidæ, as in the Helioporidæ, the bulk of the cœnenchymal mesoglœa and the whole of the calcigenous elements are derived from the ectoderm. In the one case the mesoglœal elements preponderate greatly over the calcigenous, in the other the preponderance of the calcigenous elements has led to the formation of a dense calcareous skeleton, the mesoglœal elements being reduced to a very subordinate position.

XX. "Degenerations consequent on Experimental Lesions of the Cerebellum." By J. S. RISIEN RUSSELL, M.D., M.R.C.P., Assistant Physician to the Metropolitan Hospital. Communicated by Professor V. HORSLEY, F.R.S. Received June 4, 1894.

(From the Pathological Laboratory of University College, London.)

(Abstract.)

The paths which degenerate after ablation of one lateral lobe of the cerebellum, and after extirpation of its middle lobe, are discussed in this paper.

The former operation, viz., removal of one lateral lobe of the cerebellum, results in degeneration of all the peduncles on the side of the lesion, and in the superior peduncle of the opposite side; but no fibres degenerate in the middle or inferior peduncle of the opposite side. The degenerated fibres in the superior peduncle on the side

of the lesion decussate in the posterior quadrigeminal region, and pass to the opposite red nucleus and optic thalamus. None could be traced beyond this point. Those in the opposite superior peduncle represent fibres which degenerate in the cerebellum, passing from the seat of lesion across to the intact half of the organ, and leaving it by this peduncle. These degenerated fibres occupy a special position in the peduncle, a part of it which is comparatively free from degenerated fibres on the side of the lesion, and a part occupied by degenerated fibres on both sides, when the cerebellum is divided into two lateral halves by a mesial incision. These facts are held to controvert Marchi's statement, that none of the peduncles contain commissural fibres.

The degenerated fibres in the middle peduncle, on the side of the lesion, pass chiefly to the grey matter of the opposite side of the pons. Some degenerated fibres from this source pass between the pyramidal bundles, but there is no evidence to support Marchi's observation, that degenerated fibres also pass from this peduncle in the fillet and posterior longitudinal bundle to the corpora quadrigemina and periphery of the antero-lateral region of the spinal cord, and that some pass to the corpus striatum by way of the pyramidal tract.

Of the fibres which degenerate in the inferior peduncle, the majority occupy the lateral region of the medulla, becoming more and more scattered as they pass down. These can no longer be said to form a tract below the level of the superior pyramidal decussation; but a few scattered degenerated fibres occupy the antero-lateral region of the cervical cord, beyond which none can be traced. Degenerated fibres pass to both inferior olives from this peduncle; but no well-marked tract to the opposite inferior olive, as described by Ferrier and Turner, was found. In accordance with these observers, however, no corroboration of Marchi's results was found, in so far as he states that degenerated fibres pass from this peduncle to the ascending root of the fifth, the roots of the cranial nerves through the posterior longitudinal bundles, and the spinal nerves by the descending antero-lateral tract.

In confirmation of Marchi, and contrary to the observations of Ferrier and Turner, degenerated fibres were found in all the peduncles of the cerebellum, after extirpation of its middle lobe. Those in the superior peduncle occupy all parts of it, as seen on transverse section, they decussate in the region of the posterior corpora quadrigemina, and terminate in the opposite red nucleus, beyond which point no degenerated fibres could be traced.

The degenerated fibres in the middle peduncle behave much as do those which result from ablation of one lateral lobe of the cerebellum, and the same may be said with regard to the degenerated fibres in the inferior peduncle. No evidence was found to support Marchi's

statement that degenerated fibres from this source pass to the cranial nerve roots through the posterior longitudinal bundles, and to the antero-lateral columns of the cord by way of the fillet.

With regard to the well-marked antero-lateral tract, which Marchi describes as degenerating throughout the whole length of the spinal cord, it is held, in conjunction with Ferrier and Turner, that no such tract degenerates after lesions limited to the cerebellum. And in support of this negative view being probably the correct one, is adduced the fact that Ferrier and Turner found a similar tract after injury to Deiter's nucleus, as did Mott also, after injury to the posterior column nuclei.

XXI. "A Contribution to the Study of (i) some of the Decussating Tracts of the Mid- and Inter-brain, and (ii) of the Pyramidal System in the Mesencephalon and Bulb." By RUBERT BOYCE, M.B., Assistant Professor of Pathology in University College, London. Communicated by Professor VICTOR HORSLEY, F.R.S. Received June 9, 1894.

(From the Pathological Laboratory of University College, London.)

(Abstract.)

The present paper is supplementary to a paper communicated to the Royal Society, February, 1894, entitled a "Contribution to the Study of the Descending Degenerations in the Brain and Spinal Cord." It is based upon a study of the changes found in the brains and spinal cords of the animals (cats) used for that research.

1. It is found that hemisections of the mesencephalon through the superior quadrigeminal region is followed by degeneration of *Meynert's commissure* and *Forel's decussation*, situated in front of the third ventricle and behind the optic chiasma.

The degenerate fibres which go to form the *decussation of Forel* are large medullated fibres which ascend from the seat of injury in the tegmental region, proceed forwards and anteriorly, and then curve round in front of the third ventricle, between the latter and Meynert's commissure. They then pass backwards, between the optic tract and the internal capsule (*pes pedunculi*), and appear to end in the lateral thalamic region. This description agrees with that given by Darkschewitch and Pribytkow, who, however, state that the fibres terminate in the lenticular nucleus; by the Marchi method, on the other hand, the Author has traced the fibres past this nucleus, and across the internal capsule into the thalamus.

The fibres appear to be part of the fibres constituting the "fountain (ventral) decussation of Forel."

Meynert's Commissure.—This commissure is also invariably found degenerate, but the author has been unable to determine its exact mode of origin and termination. It would appear that the commissure had a wide field of origin, numerous fibres either passing through or arching round the *pes pedunculi* on its dorsal aspect to form it. The fibres pass to the opposite side behind the chiasma, and then descend slightly, and appear to diminish in number; they do not appear to enter the *corpus Luysii*; a few of the fibres may penetrate with the optic tract into the thalamic region, and intermingle with the superficial fibres of the superior fillet (compare Darkschewitch and Pribytkow, and more recently Bechterew in "Die Leitungsbahnen").

2. *Posterior Commissure.*—The degenerate fibres which cross in the commissure or in the roof of the Aqueduct of Sylvius, and which result from a complete unilateral lesion of the quadrigeminal area, have not a long course, but terminate, for the most part, in the opposite corpora quadrigemina, dorsal and lateral aspects of the Sylvian grey matter, or posterior portion of the tegmentum.

Degenerate fibres have never been traced into the posterior longitudinal bundles, as has been asserted by some authors. A special group of large superficial degenerate fibres in the anterior portion of the roof of the aqueduct have been traced from the internal capsule across the thalamus into the stalk of the superior corpus quadrigeminum and then across the commissure. These fibres alone are found degenerate in the commissure when the anterior one-third of the cat's hemisphere is removed.

3. In cases where the motor region is completely removed in the cat, degenerate fibres are found which leave the pyramidal system in the pes pedunculi, crusta, pons, and medulla. The fibres which leave the pes pedunculi and crusta pass backwards to the quadrigeminal region of the same side, those which leave the pyramid in the medulla decussate across the raphe to the opposite side, and lose themselves in the tegmentum; they have not been traced directly ending in the motor nuclei of the cranial nerves. Muratoff has described a group of these fibres in the medulla, and supposes that they are the cortical motor fibres of the VIIth; the author, on the other hand, has not found the fibres limited alone to this region. The decussation of the pyramid is thus not confined to the upper cervical region, but is gradually taking place during the descent of the pyramid through the bulbar segments.

XXII. "A Magnetic Survey of the British Isles for the Epoch January 1, 1891." By A. W. RÜCKER, F.R.S., and T. E. THORPE, F.R.S. Received June 21, 1894.

[Publication deferred.]

XXIII. "On the Different Forms of Breathing." By WILLIAM MARCET, M.D., F.R.S. Received June 12, 1894.

[Publication deferred.]

The Society adjourned over the Long Vacation to Thursday, November 15.

Presents, June 21, 1894.

Transactions.

Baltimore:—Johns Hopkins University. Circulars. Vol. XIII. No. 112. 4to. *Baltimore* 1894. The University.

Berlin:—Deutsche Chemische Gesellschaft. Berichte. 1893. No. 19. 1894. Nos. 1—9. 8vo. *Berlin.* The Society.

Berne:—Naturforschende Gesellschaft. Mittheilungen. 1893. 8vo. *Bern* 1894. The Society.

Bombay:—Bombay Branch of the Royal Asiatic Society. Journal. Vol. XVIII. Nos. 49a—50. 8vo. *Bombay* 1894. The Branch.

Brussels:—Académie Royale de Médecine. Bulletin. Tome VII. Nos. 10—11. Tome VIII. Nos. 1—4. 8vo. *Bruxelles* 1893–94. The Academy.

Académie Royale des Sciences. Bulletin. Tome XXVII. Nos. 1—4. 8vo. *Bruxelles* 1894. The Academy.

Calcutta:—Asiatic Society of Bengal. Journal. Vol. LXII. Part 1. No. 4. Part 2. No. 4. 8vo. *Calcutta* 1893; Proceedings. 1893. No. 10. 1894. No. 1. 8vo. *Calcutta* 1894; Annual Address, delivered by the Hon. Sir C. A. Elliott. 1894. 8vo. *Calcutta* 1894. The Society.

Cambridge, Mass.:—Harvard University. Bulletin. Vol. VII. No. 6. 8vo. *Cambridge, Mass.* 1894. The University.

Dublin:—Royal Irish Academy. Transactions. Vol. XXX. Parts 11—12. 4to. *Dublin* 1894; Proceedings. Vol. III. No. 2. 8vo. *Dublin* 1894. The Academy.

Florence:—Biblioteca Nazionale Centrale. Bollettino. 1891. Indici. 1894. Nos. 193—202. 8vo. *Firenze.* The Library.

Transactions (*continued*).

Kazan:—Imperial University. Scientific Notes. 1894. No. 3. [*Russian.*] 8vo. *Kazan.* The University.

Leipsic:—Astronomische Gesellschaft. Vierteljahrsschrift. Jahrg. XXIX. Heft 1. 8vo. *Leipzig* 1894. The Society.

London:—British Association for the Advancement of Science. Report of the Sixty-third Meeting, 1893. 8vo. *London* 1894. The Association.

British Museum (Natural History). Catalogue of the Birds. Vol. XXIII. 8vo. *London* 1894; Catalogue of the Mesozoic Plants. Part 1. 8vo. *London* 1894; A Monograph of the Lichens found in Britain: being a Descriptive Catalogue of the Species in the Herbarium. Part 1. 8vo. *London* 1894. The Museum.

Camera Club. Journal. Vol. VIII. No. 97. 8vo. *London* 1894. The Club.

Chemical Society. Journal. January—June, 1894. 8vo. *London;* Proceedings. Nos. 131—139. 8vo. *London* 1894. The Society.

Geological Society. Abstract of the Proceedings. Nos. 616—627. 8vo. [*London* 1894.] The Society.

Institution of Civil Engineers. Abstracts of the Proceedings. Session 1893–94. Nos. 6—16. 8vo. [*London.*] The Institution.

Institution of Electrical Engineers. Journal. Vol. XXII. Nos. 108—111. 8vo. *London* 1894. The Institution.

Middlesex Hospital. Reports of the Medical and Surgical Registrars, and Pathologist for the year 1892. 8vo. *London* 1894. The Hospital.

Odontological Society. Transactions. Vol. XXVI. No. 7. 8vo. *London* 1894. The Society.

Pharmaceutical Society of Great Britain. Pharmaceutical Journal and Transactions. January—June, 1894. 8vo. *London.* The Society.

Royal Astronomical Society. Monthly Notices. Vol. LIV. Nos. 2—6. 8vo. *London* 1894. The Society.

Royal Geographical Society. The Geographical Journal. January—June, 1894. 8vo. *London.* The Society.

Royal Institution of Great Britain. Reports of the Weekly Evening Meetings. May, 1893. March—April, 1894. 8vo. *London.* The Institution.

Society of Arts. Journal. January—June, 1894. 8vo. *London.* The Society.

Society of Chemical Industry. Journal. Vol. XII. No. 12. Vol. XIII. Nos. 1—4. 8vo. *London* 1894. The Society.

Transactions (*continued*).

Zoological Society. Proceedings. 1894. Part 1. 8vo. *London.* The Society.

Lyons:—Société d'Anthropologie. Bulletin. Tome XII. 8vo. *Lyon* 1894. The Society.

Société Linnéenne. Annales. Tome XXXVIII—XL. 8vo. *Lyon* 1891–93. The Society.

New York:—American Museum of Natural History. Bulletin. Vol. VI. Pages 161—192. 8vo. *New York* 1894. The Museum.

Paris:—Académie des Sciences. Comptes Rendus. Janvier—Juin, 1894. 4to. *Paris.* The Academy.

École des Hautes Études. Bibliothèque. Fasc. 100. Livr. 1. 8vo. *Paris* 1893. The School.

École Normale Supérieure. Annales Scientifiques. Tome XI. No. 6. 4to. *Paris* 1894. The School.

Muséum d'Histoire Naturelle. Tome V. 4to. *Paris* 1893; Centenaire de la Fondation du Muséum d'Histoire Naturelle (10 Juin, 1793—10 Juin, 1893): Volume Commémoratif. 4to. *Paris* 1893. The Museum.

Société de Biologie. Comptes Rendus. Janvier—Juin, 1894. 8vo. *Paris.* The Society.

Société de Géographie. Comptes Rendus des Séances. 1894. Nos. 1—12. 8vo. *Paris.* The Society.

Société d'Encouragement pour l'Industrie Nationale. Bulletin. Novembre—Décembre, 1893. Janvier—Avril, 1894. 4to. *Paris;* Compte-Rendu des Séances. Janvier—Juin, 1894. 8vo. *Paris.* The Society.

Société Française de Physique. Bulletin Bimensuel. Janvier—Juin, 1894. 8vo. *Paris.* The Society.

Société Géologique. Bulletin. Tome XXI. No. 5. Tome XXII. No. 1. 8vo. *Paris* 1894; Compte-Rendu des Séances. 1894. Nos. 1—11. 8vo. [*Paris.*] The Society.

Société Philomathique. Compte-Rendu des Séances. 1894. Nos. 6—14. 8vo. *Paris.* The Society.

Philadelphia:—Academy of Natural Sciences. Proceedings. 1893. Pp. 377—520. 1894. Pp. 1—88. 8vo. [*Philadelphia.*] The Academy.

Franklin Institute. Journal. January—June, 1894. 8vo. *Philadelphia.* The Institute.

Prague:—Gesellschaft zur Förderung deutscher Wissenschaft, Kunst und Literatur in Böhmen. Mittheilung. Nr. 2. 8vo. *Prag* 1894. The Society.

Rome:—Accademia Pontificia de' Nuovi Lincei. Atti. Anno XLVI. Sessioni 1—3. 4to. *Roma* 1893. The Academy.

Transactions (*continued*).

Reale Accademia dei Lincei. Rendiconti. Serie 5. Vol. III. Semestre 1. Fasc. 1—8. 8vo. *Roma* 1894. The Academy.

Siena:—R. Accademia dei Fisiocritici. Atti. Vol. VI. Fasc. 6—7. 8vo. *Siena* 1894; Processi Verbali. No. 4. 8vo. *Siena* 1894. The Academy.

Stockholm:—Kongl. Vetenskaps Akademie. Öfversigt. Arg. LI. No. 4. 8vo. *Stockholm* 1894. The Academy.

Switzerland:—Société Helvétique des Sciences Naturelles. Actes. 1893. 8vo. *Lausanne;* Compte Rendu des Travaux. 1893. 8vo. *Genève.* The Society.

Sydney:—Linnean Society of New South Wales. Abstract of Proceedings. March—April, 1894. 8vo. *Sydney.* The Society.

Toulouse:—Académie des Sciences, Inscriptions et Belles-Lettres. Mémoires. Série 9. Tome I. 8vo. *Toulouse* 1889. The Academy.

Venice:—R. Istituto Veneto di Scienze, Lettere ed Arti. Atti. Tomo L. Disp. 4—10. Tomo LI. Tomo LII. Disp. 1—3. 8vo. *Venezia* 1892–94. The Institute.

Vienna:—Kais. Akademie der Wissenschaften. Sitzungsberichte (Math.-naturw. Classe). Bd. CIII. Abth. 2a. Heft 1—2. Abth. 2b. Heft 1—3. 8vo. *Wien* 1894; Anzeiger. Jahrg. 1894. Nos. 1—13. 8vo. *Wien.* The Academy.

Observations and Reports.

Dublin:—General Register Office. Weekly Return of Births and Deaths. January—June, 1894. 8vo. *Dublin.* The Office.

Greenwich:—Royal Observatory. Rates of Chronometers on trial, from July, 1893, to January, 1894. 4to. [*London* 1894]; Rates of Deck Watches on trial, from October, 1893, to February, 1894. 4to. [*London* 1894]; Report of the Astronomer Royal to the Board of Visitors, June 2, 1894. 4to. [*London.*] The Observatory.

India:—Great Trigonometrical Survey. Catalogue of Stars for the Epoch January 1, 1892. 4to. *Dehra Dun* 1893. The Survey.

London:—Meteorological Office. Daily Weather Reports. January 1—February 17. 4to. *London;* Weekly Weather Report. Vol. XI. Nos. 1—21. 4to. *London* 1894. The Office.

Melbourne:—Observatory. Records of Results of Observations in Meteorology and Terrestrial Magnetism. January—September, 1893. 8vo. *Melbourne* 1893–94. The Observatory.

Observations and Reports (*continued*).

Paris:—Observatoire. Rapport Annuel, 1893. 4to. *Paris* 1894. The Observatory.

Washington:—Department of Agriculture. Monthly Weather Review. March, 1894. 4to. *Washington*. The Department.

U.S. Patent Office. Official Gazette. Vol. LVI. Vol. LVII. Nos. 1—8. 8vo. *Washington* 1894; with Alphabetical Lists of Patentees and Inventions. The Office.

Journals.

American Chemical Journal. Vol. XVI. Nos. 1—5. 8vo. *Baltimore*. The Editor.

American Journal of Mathematics. Vol. XVI. Nos. 1—2. 4to. *Baltimore* 1894. The Editors.

American Journal of Science. January—June, 1894. 8vo. *New Haven*. The Editors.

Analyst. January—June, 1894. 8vo. *London*. Society of Public Analysts.

Annalen der Physik und Chemie. 1894. Nos. 1—6. 8vo. *Leipzig*; Beiblätter. 1893. No. 12. 1894. Nos. 1—5. 8vo. *Leipzig*. The Editors.

Annales des Mines. 1893. Livr. 12. 1894. Livr. 1—4. 8vo. *Paris*. École des Mines, Paris.

Annales des Ponts et Chaussées. 1893. Cahier 10—12. 1894. Cahier 1—4. 8vo. *Paris*. Ministère des Travaux Publics, Paris.

Archives des Sciences Biologiques. Tome II. No. 5. 4to. *St. Pétersbourg* 1893. Institut Impérial de Médecine Expérimentale.

Astronomical Journal. Vol. XIV. Nos. 1—6. 4to. *Boston (Mass.)* 1894. Smithsonian Institution.

Astronomie (L') Janvier—Juin, 1894. 8vo. *Paris*. The Editor.

Astronomy and Astro-Physics. June, 1894. 8vo. *Northfield, Minn.* The Editors.

Ateneo Veneto. Serie XVI—XVII. 8vo. *Venezia* 1892–93. The Ateneo.

Athenæum. January—June, 1894. 4to. *London*. The Editor.

Builder. January—June, 1894. Folio. *London*. The Editor.

Chemical News. January—June. 1894. 8vo. *London*. Mr. W. Crookes, F.R.S.

Cosmos. Janvier—Juin, 1894. 8vo. *Paris*. The Editor.

Journals (*continued*).

Educational Times. January—June, 1894. 4to. *London.*
College of Preceptors.

Electrical Engineer. January—June, 1894. Folio. *London.*
The Editor.

Electrical Review. January—June, 1894. Folio. *London.*
The Editor.

Electrician. January—June, 1894. Folio. *London.*
The Editor.

Electricien (L'). Janvier—Juin, 1894. Folio. *Paris.*
The Editor.

Industries and Iron. January—June, 1894. 4to. *London.*
The Editor.

Meteorologische Zeitschrift. 1893. Heft 12. 1894. Hefte 1—5. Sm. Folio. *Wien.*
Oesterreichische Gesellschaft für Meteorologie.

Morskoi Sbornik. 1893. Nos. 10—12. 1894. Nos. 1—2. [*Russian.*] 8vo. *St. Petersburg.*
Compass Observatory, Cronstadt.

Nature. January—June, 1894. Roy. 8vo. *London.* The Editor.

New York Medical Journal. January—June, 1894. 4to. *New York.* The Editor.

Notes and Queries. January—June, 1894. 4to. *London.*
The Editor.

Observatory. January—June, 1894. 8vo. *London.* The Editors.

Revue Générale des Sciences. Janvier—Juin, 1894. 8vo. *Paris.*
The Editor.

Revue Scientifique. Janvier—Juin, 1894. 4to. *Paris.*
The Editor.

Symons's Monthly Meteorological Magazine. January—June, 1894. 8vo. *London.* Mr. G. J. Symons, F.R.S.

Arnoux (G.) Arithmétique Graphique: les Espaces Arithmétiques Hypermagiques. 8vo. *Paris* 1894. The Author.

Döllen (W.) Stern-Ephemeriden, 1892–93. 8vo. *Berlin, Dorpat* 1891—93. The Author.

Ebermayer (E.) Die gesammte Lehre der Waldstreu mit Rücksicht auf die Chemische Statik des Waldbaues. 8vo. *Berlin* 1876; Die physikalischen Einwirkungen des Waldes auf Luft und Boden und seine klimatologische und hygienische Bedeutung. 8vo. *Berlin* 1873 [and Atlas. Folio].
Sir John Evans, Treas. R.S.

Norman (J. H.) The Science of Money. 8vo. *London* 1894.
The Author.

Pavy (F. W.), F.R.S. The Physiology of the Carbohydrates; their Application as Food and Relation to Diabetes. 8vo. *London* 1894. The Author.

Rambaut (A. A.) Occultation of *α* Virginis observed at Dunsink, 1894, March 22. 8vo. *London* 1894; On the Great Meteor of February 8, 1894. 8vo. *Dublin* 1894; On the Proper Motion of Stars in the Dumb-bell Nebula. 8vo. *London* 1894. The Author.

Saint-Lager (Dr.) Onothera ou Œnothera: les Anes et le Vin. 8vo. *Paris* 1893. The Author.

Five Photographs of Nebulæ. Dr. Isaac Roberts, F.R.S.

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NOVEMBER 24, 1894.

Third Report to the Royal Society Water Research Committee.

By PERCY F. FRANKLAND, Ph.D., B.Sc., F.R.S., Professor of Chemistry in Mason College, Birmingham, and H. MARSHALL WARD, D.Sc., F.R.S., F.L.S., F.R.H.S., Professor of Botany, Royal Indian Engineering College, Cooper's Hill. Presented to the Committee October 4, 1894.

PART I.

"Further Experiments on the Action of Light on *Bacillus anthracis*, and on the Bacteria of the Thames." By H. MARSHALL WARD, D.Sc., F.R.S., F.L.S., F.R.H.S., Professor of Botany, Cooper's Hill.

In a previous Report to the Committee, I have shown that the action of light on bacteria is not only very definite, and much more pronounced than had hitherto been supposed, but that it has an importance in its bearing on the question of the destruction of these organisms in the water of rivers, ponds, &c., vastly greater than had ever been suspected.

In this Report I offer some of the results of long continuous investigations into the details of this bactericidal action of light—both solar and electric—and into the bacterial flora of the River Thames, as studied at a point where it flows below Cooper's Hill.

The reader may be referred to the previous Reports* for details as to the methods of investigation employed, and as to the chief results obtained by exposing the spores of *Bacillus anthracis* to the direct action of undecomposed solar light; he will also find further details in two papers presented to the Royal Society in 1892 and 1893† regarding this matter, and regarding preliminary investigations into the action of solar light which has been passed through various absorbent media.

Special attention may be directed to pp. 23—34 of the 'Proceedings,' vol. 53, and the following experimental results may in the first place be taken as supplementing those there published.

* 'Proc. Roy. Soc.,' vols. 51 and 53.

† 'Proc. Roy. Soc.,' vol. 52, pp. 393—400; and vol. 53, pp. 23—44.

Table A.—Experiments with Spores and Media separate.

Number of plate.	Nature of plate.	When made.	When exposed.	Number of hours sunlight.	When put to incubate.	Number of days incubated.	Results, indicating the figure or letter used.	Remarks.
D (1)	Agar only	Feb. 12	Feb. 12 and 13	5 (partly fitful)	..	8	No trace whatever of the letter Y, the spores germinated evenly all over	In these cases the *exposed* slab of agar was placed on a dry film of *unexposed* spores before incubation.
E (1)	„	..	„	„	..	5	„	
F (1)	Spores only	..	„	„	..	5	No results beyond a dim "ghost" of a letter, owing to overgrowth	I made the mistake of adding a little water after putting a slab of unexposed agar on the spores; the water ran in and flooded the plate.
H (1)	„	..	Feb. 13	3 (fitful)	..	8	Good letter U, but developed very slowly	The extreme slowness of development here was due to my using large slabs of agar, and therefore obstructing aeration.
I (1)	„	..	„	„	..	8	Good letter T, very slow	
J (1)	Agar only	..	„	„	..	8	No trace of letter C.	
A	Spores only	Feb. 18	Feb. 19	6 (reflected)	Feb. 20	1	Excellent and sharp W	
B	„	„	„	„	lost			
C	Agar only	„	„	„	Feb. 20	4	No trace of letter Z.	
D	„	„	„	„	„	4	„ „	
E	Spores only	„	Feb. 19 and 20	5*	Feb. 21	1	Good sharp E........	* Behind glass screen (ordinary 10).
F	„	„	„	5	„	2	Letter X distinct......	Developed slowly at first, but sharp and clear afterwards.
G	„	„	„	5†	„	1	Sharp letter H........	† Behind blue glass screen (Blue 1).

H	"	"	Feb. 25	5‡	Feb. 25	1	Perfectly clear E......	‡ Behind an alum screen ¼ in. thick. The interval between 18th—25th February was dull and cold.
D	Spores and agar block	Mar. 20	Mar. 21	3½	Mar. 21	17 hours	The letters (B and C) visible at 9 A.M. on 22nd, and *sharp* at 5 P.M.	Very hot, brilliant sun and blue sky. Excellent letters on 23rd, but not all the spores killed.
F (*a*)	Spores only	"	"	2¾	"	6 days	Traces, 5 P.M. on 22nd, of T	No further results. No trace of T after.
F (*b*)	Spores and agar block	"	"	"	"	6 "	Traces of ✠ at 5 P.M. on 22nd	Not all killed, but good cross.
G	Spores only	"	"	3	"	23½ hours	Just visible, 3 P.M. 22nd	No further result. No trace on 24th; kept till 27th.
H	Spores and agar blocks	"	"	3	"	6 days	Cleared area only......	Obliterated later.

§ I.

Before proceeding to the results obtained by exposure behind coloured screens, I select the following series of further experiments, which confirm some of my previous statements, referred to above (Table A).

It is worth remark that these exposures were made in February, at a time when the temperature was low, and the sunlight, though bright, of an intensity far below that obtainable in the summer. The methods of preparing the agar plates and films of dried spores, have already been described, that is to say, in experiments numbered D (1), E (1), J (1), C and D, a plate of sterile agar was exposed, in each case behind a stencil plate, and, after exposure, was laid flat on a film of dried unexposed spores of *B. anthracis*, whereas in the cases marked otherwise it was the film of spores which was thus exposed, and a sterile plate of unexposed agar then placed on the film. Incubation then decided whether the light had produced any effect, the results being given in the table.

These results fully confirm those obtained previously, and show that the action of the light is direct on the spores, and not on the food material—in this case agar—in which the spores are suspended. That the slow development in the cases marked H (1) and I (1) was due to deficient aeration—possibly in part also to the fitful sunshine to which the plates were exposed—is borne out by the following experiments. Three stout glass tubes were selected, sterilised, and charged each with about 5 c.c. of bouillon, in which was distributed a small loop-full of the spores of *B. anthracis*. Each tube was about 6 in. long, and, after charging, was plugged with sterilised cotton wool, the plug being pushed 2 in. into the tube. Each tube was then drawn out to a point, exhausted of air, and the end sealed in a flame. The vacuum tubes were kept thus sealed until the following day, when two of them were broken at the tips, in a flame, to let in air; the other remained sealed.

The sealed tube, and one of the now unsealed tubes, were then exposed all day to a bright sun, while the third (unsealed) tube was wrapped in tin-foil and black paper, and placed side by side with the others, thus protected from the light. After six or seven hours' exposure, the tip of the still sealed tube was also broken, and all three placed in the incubator at 22° C.

In forty-eight hours the covered tube and the exposed sealed tube were equally and copiously turbid, with a vigorous growth of the bacillus; the exposed *unsealed* tube showed the faintest traces only of this turbidity.

The inference is obvious. Exposure to sunlight *in vacuo* results in no perceptible retardation or destruction of the bacillus, whereas if

exposed in contact with air nearly all the spores are killed in the time given. As will be shown later, the thickness of the glass of which the tubes were composed is no doubt an important factor, and probably *all* the spores in the exposed unsealed tube would have been killed had the glass been thinner, as they certainly could have been by a longer exposure.*

§ II.

The following series of experiments were carried out during February and March of this year (1893) to obtain some information as to the time of exposure necessary to kill the spores of *Bacillus anthracis*, for I found it desirable to make myself as well acquainted as possible with the power of the solar rays in this respect, in order to utilise the experience in succeeding work. As the table shows, I also tried comparisons between the action of direct and that of reflected sunlight. As the experiments proceeded, it turned out that several difficulties have to be met in attempting to compare the action on two or more different plates exposed side by side.

It seems impossible to ensure absolute similarity between any two plates, for the following reasons:—

1. The difficulty of distributing the spores in equal quantities, and at equal distances apart in the agar. The best results were obtained by pouring the agar on all the plates from one large tube, in which the infected melted agar is thoroughly shaken; but even then it was impossible to be sure that the agar film in each plate was of equal thickness. Of course practice and experience enable one to pour approximately the same quantity of the agar into each plate, but this does not entirely overcome the difficulty.

2. Even very careful selection from a large number of the Petri's dishes does not secure that each dish used shall have a perfectly plane glass face (to be exposed), of equal thickness, and identical in its properties towards the light. Here, again, therefore, one had to be satisfied with as close approximations as possible.

It was owing to these difficulties that I hit upon the device of employing one plate with several square or circular "windows" cut in its covering, at equal distances apart. After exposing all the "windows" for, say, half an hour, one was then covered; after a further exposure of half an hour, a second one was covered, and so forth (or, conversely, the windows uncovered in succession), as in the cases marked I*a* to I*d*, and 5*a* to 5*d*, &c., on Table B.

But another difficulty now made itself evident, namely, that as the intensity of the solar light may vary considerably from time to time not only owing to altitude, but also to differences in the atmosphere,

* It may be pointed out that these results confirm those of Roux, 'Ann. Past. Inst.,' 1887.

Table B.—Experiments with Spores in Agar, without Screens.

Number of plate.	Nature of plate.	Date made.	When exposed.	Number of hours sunshine.	When put to incubate.	Number of days incubated.	Results.	Remarks.
A (1)	Agar-spores	Feb. 12	Feb. 12	3	Feb. 12	3	No distinct letter E came out	The sunlight fitful between clouds.
B (2)	„	„	Feb. 12	2	„	3	Good letter Z, though the plate had slipped	Sunlight interrupted by clouds.
T	„	Feb. 28	Feb. 28	4	Feb. 28	4	No trace of germination anywhere on plates	
7	„	Mar. 4	Mar. 5	1	Mar. 5	6	Only half-a-dozen colonies	Direct sunlight.
8	„	„	„	1	„	6	A dozen colonies	Reflected sunlight.
9	„	„	„	4	„	6	Only a couple of colonies.	Direct sunlight. } Only the first hour direct sun, the other three bright, diffused, cloudy.
10	„	„	„	4	„	6	A few colonies only....	Reflected sunlight. } Only the first hour direct sun, the other three bright, diffused, cloudy.
13	„	Mar. 7	Mar. 7	2	Mar. 7	2	Sharp B, but 20—30 colonies on it	Reflected sunlight. } Cloudy, with hot glare, & gleams of bright sun.
14	„	„	„	2	„	2	Good C, but less sharp edges; about same number of colonies	Direct sunlight. } Cloudy, with hot glare, & gleams of bright sun.
I*a*	„	Mar. 10	Mar. 10	$\frac{1}{2}$	Mar. 10	2	Merest ghost of square	Successive squares on same plate and direct light.
I*b*	„	„	„	1	„	2	Good square, but not *all* killed	Successive squares on same plate and direct light.
I*c*	„	„	„	$1\frac{1}{2}$	„	2	Square quite clear, and sharp	Successive squares on same plate and direct light.
I*d*	„	„	„	2	„	2	Square quite clear, and sharp	Successive squares on same plate and direct light.

IV*b*	"	"	"	1	"	1	Cross visible in 20 hours, and very sharp in 45 hours	A ✠ cut in plate over mirror.
VII*a*	"	Mar. 11	Mar. 11	2	Mar. 11	6	No trace of germination	Letters B, C on same plate. C exposed three hours and B only two to direct sun. Internal reflection (?) inhibited.
VII*b*	"	"	"	3	"	6	Ditto	
VIII	"	"	"	3	"	6	Centre clear, and in a somewhat vague Y-shape at last	Direct sun. Letter Y.
IX	"	"	"	3	"	2	Sharp X in 40 hours	Reflect sun. The X is nearly clear—only about a dozen colonies on it—on third day.

Table B—*continued.*

Number of plate.	Nature of plate.	Date made.	Date exposed.	Kind of exposure.	Hours' sun.	Into incubator.	Hours incubated at 25° C.	Results.	Remarks.
5*a*	..	Mar. 13	Mar. 13	Direct	1.45—4.15	4.30 P.M. Mar. 13	40 hours	Window distinct, but only about half the spores killed.	A four-windowed plate. Sun obscured by cloud, and very dull after 2.45 P.M.
5*b*	..	,,	,,	,,	2.15—4.15	,,	,,	Window *just* perceptible	
5*c*	..	,,	,,	,,	2.45—4.15	,,	4 days	No trace: germinated all over	
5*d*	..	,,	,,	,,	3.15—4.15	,,	,,		
XVIII	Ag. sp.	Mar. 27	Mar. 27	,,	12.15—3.15	3.15 P.M. Mar. 27	,,	In 18 hours = excellent Y, rapidly sharpening up to 5 P.M. On 29th best letter seen. Nearly cleared	Very hot sun, hazy first hour, then brilliant.
XX A	,,	,,	,,	,,	12.30—2.30	3.45 on Mar. 27	,,	Could detect little or no difference between A, B, D, and E, and C = spoilt owing to contact with lid.	
XX B	,,	,,	,,	,,	12.30 —3.45	,,	,,		
XX C	,,	,,	,,	,,	12.30—1.30	,,	,,		
XX D	,,	,,	,,	,,	12.30—3.45	,,	,,		
XX E	,,	,,	,,	,,	12.30—3.30	,,	,,		

and to clouds passing, &c., &c., it seemed utterly hopeless to expect the accurately comparative results required.

Taking all these drawbacks into consideration, the Table B nevertheless shows some significant and instructive facts.

The experiment denoted I*a*, for instance, shows that, even in March, the solar action can be detected clearly after so short an exposure as half an hour to an hour, while exposures of $1\frac{1}{2}$ to 2 hours resulted in sharp, clear figures.

After a large number of these comparative trials, however, I concluded (1) that while it seems impossible to overcome all the difficulties, and to express the nature of the exposure in words, the general impression gathered was that on certain bright, sunny days in the spring—days when the sky is blue and cloudless, and the air peculiarly clear, the bactericidal power of the direct or reflected solar rays is very great—much greater than has been supposed. A very slight amount of haze makes a vast difference in the times of exposure (*e.g.*, Cases A (1), and B (1), where a much better result was obtained in two hours in the one case than with three hours in the other); (2) that very long exposures are necessary if the sky is overcast with clouds, even though the light is otherwise bright. In other words, the direct rays of the sun are needed for the purpose of rapid action; (3) with solar light, direct from the sun, very little if any difference can be detected between exposures where the rays fall *directly* on the plate, and where they are once reflected from a thin plane glass mirror silvered at the back.*

Summed up in the shortest terms, the conditions of exposure are practically the same as those required in ordinary photography, the chief difference being that the duration of the exposures amounts roughly to hours or half hours in the cases under consideration, instead of minutes or seconds, as in quick plate photography. All this, of course, points to the blue end of the spectrum as the effective one, a conclusion which is abundantly justified, and, in fact, fully proved in the sequel, and by my experiments with the spectrum since published.†

§ III.

In the following sets of exposures (Table C), I employed quartz, instead of glass, as a covering to the Petri's plates, so that, except in the cases 12A, 12B, and 12C, the light traversed no glass before reaching the spore-laden film.

The results show little additional information, excepting that in the cases 12D and 12E it was interesting to find that the action was approximately as pronounced after one hour of exposure as after

* This remark refers particularly to this kind of exposure.

† 'Proc. Roy. Soc.,' vol. 54, p. 472 (Abstract).

Table C.—Experiments with Quartz.

Number of plate.	Letter, &c., used.	Date made.	Date exposed.	Time of exposure.	Nature of exposure.	Date put to incubate.	Period of incubation.	Time when letter, &c., first seen.	Screen, if any.	Results.	Remarks.
11.1	□	Mar. 16	Mar. 17	9.30—10	Direct sun	12 noon, Mar. 17	28½ h.	4.30, Mar. 18	None, except the quartz	All the squares showed action.	Brilliant sun and blue sky, but windy and driving clouds. Plate with 5 square windows open on the quartz. All five squares visible at 4.30 on 18th. On the 19th the order in rank of clearness = 4 and 5 clearest : 3 : 2 and 1.
11.2	"	"	"	9.30—10.30	"	"	"	"	"		
11.3	"	"	"	9.30—11	"	"	"	"	"		
11.4	"	"	"	9.30—11.30	"	"	"	"	"		
11.5	"	"	"	9.30—12	"	"	"	"	"		
12 A	⊙	Mar. 17	Mar. 17	1.30—2.30	Reflect sun	4 P.M., Mar. 17	40 h.	9 A.M., Mar. 19	Ordy. 9	All the circular windows showed action.	Plate with 5 circular windows, of which A, B, and C had screens superposed on the quartz. Windy, and more clouds than in morning, but brilliant blue and sun at intervals. On 19th D and E (the quartz without other screen) were clearest, B next, and A least clear.
12 B	"	"	"	1.30—4.0	"	"	"	"	"		
12 C	"	"	"	1.30—4.0	"	"	"	"	Blue 1		
12 D	"	"	"	1.30—2.30	"	"	"	"	None		
12 E	"	"	"	1.30—4.0	"	"	"	"	"		
IX A	⊙	Mar. 24	Mar. 25	10.30—2.30	Direct sun	4 P.M., Mar. 25	3 days	10 A.M., Mar. 26	Quartz	All the circular windows showed action.	Hot sun, but very hazy. The sharpness and clearness of the action were directly proportional to the length of exposure, *i.e.*, C was least cleared, and B most cleared, and so on.
IX B	"	"	"	10.30—3.30	"	"	"	"	"		
IX C	"	"	"	10.30—11.30	"	"	"	"	"		
IX D	"	"	"	10.30—1.30	"	"	"	"	"		
IX E	"	"	"	10.30—12.30	"	"	"	"	"		

two and a half hours, and distinctly more so in both cases than where glass was employed in addition.

§ IV.

The following series of experiments (Table D) were made in continuation of the foregoing, and the description of glass screen employed (3rd column) refers to the table on pp. 328—329.

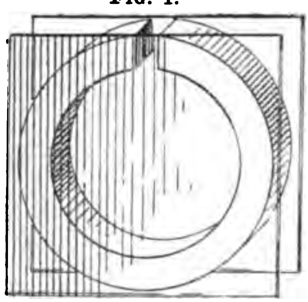
The chief feature of novelty is that I here used the *same* plate for different screens, as follows. A thick, opaque cardboard screen was prepared as large as the plates, and this screen perforated with four or five circular or square windows. Over each hole a piece of the coloured or other glass to be employed was then cemented, and the whole held in contact with the plate to be exposed, by elastic bands.

The advantage of this proceeding was that I could check the preceding results, to see if any erroneous conclusion had resulted from my using different plates.

§ V.

In order to investigate more in detail the action of the decomposed sunlight on the spores, I made a series of glass screens of the nature of water cells, or reservoirs, as follows. A number of circular, flat, indiarubber packing rings, about a quarter of an inch thick and three inches internal diameter were obtained, and a small piece cut out of each; then a thin, plain piece of glass was cemented to each side of the now incomplete ring, thus forming a reservoir with two large parallel glass ends, the sides being formed of indiarubber. I found that by carefully cementing the glass with gold size, it held very well, at least for two or three experiments, and could easily be recemented if necessary.

FIG. 1.



These glass cells were filled with the coloured transparent solutions to be referred to (see Table E), and then placed over the exposed letter on the prepared plate as described in my previous paper ('Proc. Roy. Soc.,' vol. 52, p. 393), being held in position by clips

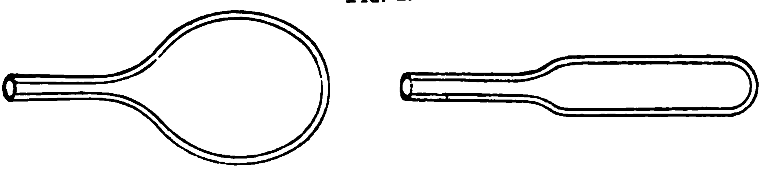
Table D.—Experiments behind other Screens.

Number of Plate.	Date made.	Nature of Screen.	Date exposed.	Time of exposure.	Nature of exposure.	Date put to incubate.	Period incubated at 25° C.	Time letter, &c., first seen.	Results.	Remarks.
6 1	Mar. 13	Ordy. 9	Mar. 13	1.45 to 4.15 P.M.	Direct	4.30 P.M. Mar. 13	40 h.	40 h.	Squares visible, but by no means all killed.	This was a 4-windowed plate arranged so that 2 were covered with glass (ordinary 9) and 2 with quartz.
6 2	„	Quartz	„	„	„	„	40 h.	40 h.	Squares possibly a trifle clearer.	
XIV 1	Mar. 27	Blue 7	Mar. 27	12—3	„	3 P.M. Mar. 27	..	18 h.	On 29th, nearly, but not quite cleared.	Very hot sun, hazy first hour, then brilliant. The plate was covered with black card excepting 5 square holes on which the screens were placed. No trace of action under orange, ruby, or olive glasses.
XIV 2	„	Orange	„	„	„	„		..	.. ..	
XIV 3	„	Ruby 5	„	„	„	„		..	.. ..	
XIV 4	„	Olive	„	„	„	„		..	.. ..	
XIV 5	„	Ordy. 10	„	„	„	„		18 h.	Somewhat more cleared than No. 1.	
XV A	„	Blue 1	„	„	„	„	..	18 h.	Very sharp and clear square.	An exactly similar plate to the last, and exposed side by side.
XV B	„	Violet	„	„	„	„		..	No trace of action at any stage.	
XV C	„	Ruby 3	„	„	„	„		..		
XV D	„	Green	„	„	„	„		..		
XV E	„	Olive	„	„	„	„		..		
XIX	„	Blue 7	„	12.15 to 3.15 P.M., Mar. 27	3.15 P.M.	„		18 h.	Capital sharp X.	

or elastic bands, and the whole then so fixed that the sunlight had to traverse the coloured or other fluid before reaching the agar film in which the spores were embedded. It will, of course, be noticed that the light here traverses three plates of glass as well as the solution of the screen before impinging on the spores in the film, a fact of importance.

I have summed up the characters and chief properties of the solutions employed in the following Table E, and need not, therefore, describe them in detail here. In all cases, excepting Nos. 3 and 5, the medium employed for solution was water; in these exceptional cases, where carbon bisulphide and alcohol were used, it was necessary to have screens devoid of cement. These were met with in the form of certain small glass flasks, shaped like brandy flasks or scent flasks, with a long neck and flat sides; they are used on the Continent for sealing up cultures of bacteria.

FIG. 2.



The chief objection to their use is that the flat sides are apt to be slightly uneven in thickness on the internal face; by carefully selecting from a large number, however, I was able to secure several very good screens of this description.* The liquid is, of course, bottled in the usual way, and the neck secured with a good cork.

§ VI.

The following Table F summarises the results of a number of exposures behind these screens of coloured and other absorbent media, with particulars as to the dates of exposure, number of hours, insolation, and incubation, and other factors worth extracting from the notes.

It will be observed that I here confined my experiments entirely to the spores of *Bacillus anthracis*. I did this because it became more and more evident that until I had obtained all the information possible about some one species—the factor most constant in this long series of slight variables—it would be difficult to value the importance of specific differences later. Experience has thoroughly confirmed the justice of this conclusion.

* In later experiments I have had the side ground out flat, and glass or quartz plates cemented on.

Table E.—Properties of the Screens Employed.

Number.	Colour.	Composition.	Light transmitted.	Rays absorbed.	Remarks.
1	Blue-violet	Ammoniacal cupric oxide	All blue-violet, from line *b* in green	Red-yellow, and part of green	A thick screen of strong solution cuts out almost all but the violet.
2	Blue-green	Prussian blue in oxalic acid	Green and blue	Part of violet, and red-yellow	
3	Purple	Iodine in carbon disulphide	Lower red and deep violet	Red-orange to violet	This superposed on amm. cupric oxide gives a screen opaque to everything below the deepest violet.]
4	Green	Methylene blue and picric acid	Lowest red and whole of green, including traces of yellow- and blue-green	Red-orange, and most of yellow; nearly all beyond green	
	Green	Chlorophyll in alcohol	Most of the red, all the orange, yellow, and green, and a little blue-green	All blue-violet, except a trace beyond green. A deep band in red, and a very faint one in green	These alcoholic chlorophyll solutions rapidly oxidise in sunlight, and become olive. Spectroscopically, the oxidised solutions let through more and more blue, till at last only violet is cut out.
6	Yellow	Picric acid......	All red to green, and a little blue-green	All blue-violet beyond beginning of blue	
7	Yellow	K. chromate (concentrated)	All red to blue, near middle of F.G.	All blue-violet beyond midway between F and G	
8	Yellow	K. chromate (dilute)	All red to blue beyond G.	Violet only, or nearly so	

9	Orange	K. bichromate ..	All red-yellow to *b* in green	All blue-violet beyond *b*	
10	Cherry-red	Eosin in water ..	All red-orange and yellow to near D	All green to violet beyond D	
11	Fluorescent	Sulphate of quinine	All visible rays......	Ultra-violet and about half violet	
12	Colourless	Alum in water ..	All visible rays......	Infra-red only	
13	Crimson	Strong fuchsin in water	Red and a little orange	All beyond red, orange	
14	Lake	Dilute fuchsin in water	Red-orange, and the violet	All from between C and D to near G	By superposing such a screen on one of amm. cupric oxide, all but the violet end can be cut out.
15	Fluorescent	Æsculin (alkaline)	Red, &c., up to half way between F and G in the blue	Cuts off all violet, and some blue	
16	Fluorescent	Æsculin + quinine sulph.	Lets a trifle more blue through	Cuts off a little less blue, but ultra-violet	
17	Colourless	Water	All visible rays......	Infra-red to some extent	

Table F.—Exposures behind

Number of plate.	Date made.	Screen employed.	Date exposed.	Kind of exposure.	Number of hours sunshine.	Date put into incubator.
C (1)	Feb. 12	Quinine sulphate	Feb. 12 and 13	Direct........	5	Feb. 13
I	„ 20	Quinine sulphate	Feb. 25	3 h. reflected: 2 h. direct	5	„ 25
J	„	Iodine in CS_2..	„	„	5	„
K	„	Prussian blue and oxalic acid	„	„	5	„
L	„	Am. cu. oxide..	„	Reflected.....	5	„
M	Feb. 25	Weak K. chromate	Feb. 25 and 26	„	3	Feb. 27
N	„	Strong potass-chromate	„	„	3	„
O	„	Picric acid	„	2 h. reflected: 1 h. direct	3	„
P	„	Methylene blue and picric acid	„	„	3	„
Q	„	Am. cu. oxide..	Feb. 26 and 28	3 h. reflected: 2 h. direct	5	Feb. 28
R	„	*Chlorophyll...	„	„	5	„
S	„	Iodine in CS_2..	„	„	5	„
U	Feb. 28	Weak K. chromate	Feb. 28	Reflected.....	4	„
V	„	Strong potass-chromate	„	„	4	„
W	„	*Chlorophyll...	„	„	4	„
X	„	Eosin.........	„	Direct........	3	„
Y	„	Quinine sulphate	„	„	3	„
1	Mar. 4	Strong potass-chromate	Mar. 4	Reflected.....	2	Mar. 4
2	„	Weak potass-chromate	„	„	2	„
3	„	Iod. + CS_2....	„	„	2	„
4	„	Eosin.........	„	„	2	„
5	„	Strong fuchsin	Mar. 5	Direct........	3	Mar. 5
6	„	Dilute fuchsin		„	3	„

Bottle Screens, Coloured, &c.

Number of days incubated.	Results.	Remarks.
5	Good letter W	The first three days showed powerful inhibition-effects, and no letter was visible till fourth day: then sharp and clear.
6	Nothing appeared on the plate till the third day, and then only about 200 colonies at the extreme margin. No letter in six days	From 20th to 25th the weather was dull and cold. Temperature of plates averaged 6° C., and none had germinated on morning of 25th, when we had brilliant hot sunshine.
6	No letter in six days	
6	No trace of germination anywhere on the plate, except extreme margin	
6	Excellent sharp letter N	
1	Letter T visible after eighteen hours incubation, but not *sharp*	The letter after three days was not very sharp, since the colonies around were large and not very numerous, and about six or eight were seen on the insolated area.
4	No letter C. Germination took place equally all over the plate	
4	Letter Y feebly visible after twenty-four hours	Even on the fourth day the contrast was not sharp, the surrounding colonies being so few and so large.
4	No letter X. Germination equal all over plate	Closer inspection showed a faint "ghost" of letter X on third day.
3	Sharp letter Z.	
3	No trace of letter	*The chlorophyll at first (Feb. 26) blocked out all the blue-violet, and had bands in red and green; but during the last two hours (Feb. 28) it only cut off violet, and had feeble bands in red and green. Colour olive, in place of deep blue-green.
3	Extremely faint B.	
3	No letter.	
3	No trace of letter.	
3	" "	*The chlorophyll = deepest solution; total absorption from *b* onwards, and deep broad bands in red and green.
3	" "	
3	Faint letter C.	
7	No trace of letter	Temperature rather high. Sun very bright and hot, but interrupted by clouds occasionally. The exposures began before 2 P.M., and ended just after 4 P.M., and two hours expresses the maximum of sunlight. All were over plane mirrors, carefully adjusted. The experiment was of little value. I had probably not used a sufficiently large charge of spores.
7	" "	
7	" "	
7	" "	
6	" "	
6	Very faint letter X	These dilute fuchsin screens need careful watching. The colouring matter gathers into flocks in time, and lets much more light through.

Table F.—Exposures behind

Number of plate.	Date made.	Screen employed.	Date exposed.	Kind of exposure.	Number of hours sunshine.	Date put into incubator.
11	Mar. 4	Strong chlorophyll	Mar. 5	Direct........	3	Mar. 5
12	„	Strong potass-chromate	„	„	3	„
15	Mar. 7	Dilute* fuchsin	Mar. 7	„	2	Mar. 7
16	„	Strong* „	„	„	2	„
17	„	Æsculin + quinine	„	Reflected.....	2	„
18	„	Æsculin.......	„	„	2	„
19	„	Potass-chromate* (strong)	„	Directed......	2	„
20	„	Quinine.......	„	Reflected.....	2	„
IIIa	Mar. 10	Iod. + CS_2....	Mar. 10	Direct........	1	Mar. 10
IIIb	„	„	„	„	2	„
IIIc	„	Strong potass-chromate	„	„	1	„
IIId	„	„	„	„	2	„
IV	„	Quinine.......	„	Reflected.....	2	„
V	„	Æsculin + quinine	„	„	2	„
VI	„	Æsculin.......	„	„	2	„
Xa	Mar. 11	Chlorophyll....	Mar. 11	„	2	Mar. 11
Xb	„	$CuSO_4$........	„	„	2	„
7	Mar. 13	Iod. + CS_2....	Mar. 13	Direct........	2½	Mar. 13
9a	„ 14	„	„ 15, 16, and 17	„	10—12	„ 17
9b	„	No screen	„	„	„	„
B 1	Mar. 21	„	Mar. 21	„	2	Mar. 21
B 2	„	CS_2 + Iod.....	„	„	2	„
C	„	Chlorophyll....	„	„	3	„
XVI	Mar. 27	KKS	Mar. 27	„	3	Mar. 27, 3 P.M.
XVII	„	Water	„	„	3	„

Bottle Screens, Coloured, &c.—*continued.*

Number of days incubated.	Results.	Remarks.
6	No trace of letter............	The chlorophyll much oxidised and olive coloured at end, but still cut out all the blue.
6	" "	
2	Good X, but by no means all killed	*Plane screen. The dilute fuchsin lets red — yellow and trace of green through, and then blocks up to one-third between F and G. Then lets a considerable proportion of blue-violet through.
4	No trace. Plate evenly covered all over	
1	Extremely faint "ghost" of Z after twenty-four hours, and invisible after forty-eight hours.	
4	No trace. Germination all over	
4	" " "	*Plane screen. This chromate is reciprocal to dilute fuchsin.
4	" " "	
7	No germination till second day. No letter	Same plate, screens, &c. Successive windows opened. Bottle screen. *Powerful* inhibition on the CS_2 side, and no germination there at all at first. Faint "ghost" after twenty-five hours on chromate side, but obliterated later.
7	No germination till third day. Very faint and transient letter later	
7	Active germination and traces of figures but transient only	
7		
2	Letter T visible in twenty hours, but not all killed.	
1	Letter X visible in seventeen hours, and sharpening up in twenty hours	Over same mirror. Both X and Z gradually obliterated next day—*i.e.*, spores not killed, only retarded.
1	Z visible, &c., *pari passu* with latter	
6	Germination equal all over plate. No letter	Extremely good sun and blue sky.
6	Good letter H.	
4	Germinated evenly all over ...	Exposed 1.45 to 4.15. Incubated at 25° C.
16½ h.	No letter appeared...........	One screened; the other not. Same plate, &c.
"	C out sharp, but diffused.....	
26 h.	Faint N, cleared by 24th, but bad outline	Same plate. H screened. N not. Very hot brilliant sun and blue sky. Kept till 25th. N clearest, but bad.
42 h.	Extremely ill-defined, and never so good on H	
6 d.	Germinated evenly all over.	
..	Much clearance over a shield-shaped area in eighteen hours, but no clear U	Very hot sun, hazy first hour, then brilliant.
..	Very sharp and clear T.	

If we now look at the results tabulated above, it is seen that the solar action is evident, though feeble, through dilute fuchsin, æsculin and quinine, and picric acid; while no trace of action occurred through potassium chromate, chlorophyll, eosin, and strong fuchsin.

On the other hand, the action was sharply defined where ammoniacal cupric oxide or water alone was employed, and also where alum dissolved in water was used. In other words, the action is most pronounced when the rays transmitted are those of the blue-violet end of the spectrum, bearing out the results already obtained more generally.

During the progress of the experiments above tabulated, a number of other points of interest were observed. With carbon bisulphide and iodine it frequently happened that no letter was obtained on the plates (Expts. J, 3, III*a*, 7, 9*a*), but occasionally the light action was recorded by the appearance of the letter (Expts. S, III*b*, B_2). The fact is, the solution did transmit a scarcely perceptible amount of violet rays, and since I could not discover any definite relation between the times of exposure and the results, one of two possibilities suggested itself—either differences in the thickness of the glass of the plates, or differences in the degree of clearness of the atmosphere may account for the discrepancies. Probably both causes were effective, for, of course, they both affect these violet rays considerably.

Another phenomenon repeatedly noticed, both in these experiments and in others, was that if the exposure to a very bright sun is continued too long, and especially if the plate is not very accurately at right angles to the direction of the rays, the light may clear the plates entirely, or nearly so. This seems to be due to the reflections of the light from the glass surfaces inside the Petri's dishes; if the light is very intense, or the exposure long, these reflected rays are sufficiently powerful to produce effects similar to those of the direct light.

This seems to me to explain another phenomenon very commonly met with. In many cases of long exposure to clear hot sunshine, the first evidence of the successful light action is not a sharp well-defined letter, but a blurred clear patch, which slowly sharpens up as incubation goes on.

It is evident that in such cases the action of the light has extended beyond the boundaries of the stencil letter, into parts of the film really not exposed to the direct incident rays. I explain this as due to the reflection of some of the rays from the glass surfaces in the interior of the plate.

These reflected rays are not sufficiently intense to complete the bactericidal action, they only inhibit the organism more or less, or at least leave many spores still alive; consequently, while these out-

lying spores germinate more slowly than those further away from the illuminated area—the stencil letter—they *do* at last germinate out, and so the previously blurred letter becomes sharp and clear in outline.

The phenomenon very much resembles the development of the indistinct "ghosts" of letters in cases where the exposure is too short, or the light not sufficiently intense, or wanting in active rays. Such faint letters gradually become obliterated as incubation proceeds, because the spores, still alive but only retarded in development, gradually germinate out to an extent so little differing from the rest that the eye fails to detect any difference.

The retarded development of a few colonies, at a late period of incubation, on the hitherto clear area of the exposed letter, is due to similar causes, but produced in a slightly different way. It is extremely difficult (probably impossible) to thoroughly distribute the spores in the film so that some do not shelter others from the light; consequently, when a clump of spores exists on the exposed area some of the inner spores may so far escape the bactericidal action as to be able to germinate out later, and I have had many experiences of these cases. In fact, the chief point about a good film—*i.e.*, one which develops a sharp letter after ordinary exposure—is that the spores shall be neither too few nor too many, and thoroughly and evenly separated and distributed; and, lastly, that the agar or other medium shall not be too thick, and thus render possible the ordering of long rows of spores one behind another (*i.e.*, in rows parallel to the ray of incident light) which thus shelter one another from the light's action.

These points, and some others, come out still more clearly in the next series of experiments.

§ VII.

The following series of experiments were made behind superposed screens, and it must be borne in mind that the light had to traverse not only a double thickness of solution, but also five thicknesses of glass, before reaching the spores.

On the whole these results may be regarded as simply confirming the previous ones, but I was (and still am) considerably puzzled by the behaviour of the iodine and carbon bisulphide screens. In several cases the plates seem to be destroyed by the light passing through this medium, and for some time I was doubtful whether there might not be a cumulative effect due to the action of the infra-red rays. It seemed extremely probable that these rays—the "dark" heat rays—do help to promote the bactericidal action, and I thought perhaps because they accelerate the chemical changes on which the action depends.

Table G.—Exposures behind Plane-screens Superposed.

Number of plate.	Letter or figure used.	Date made.	Date exposed.	Kind of exposure.	Nature of screen (plane or bottle) if any.	Number of hours sunshine.	Date put into incubator.	Number of days or hours incubated.	Time letters first visible.	Temperature incubated.	Results.	Remarks.
1	Z	Mar. 12	Mar. 12	Direct sun	Plane $CuSO_4Am$ + fuchsin	12—2 P.M.	3 P.M., Mar. 12	25½ hours	13th	25° C.	Sharp Z out at 4.30 on the 13th. Less sharp next day, growing over.	Extremely good sun, and blue sky. The screens of 1 and 3 transmitted a perceptible amount of violet, whereas that of No. 4 showed no perceptible blue or violet at all. The next experiment shows, however, that the negative result may have been due to under-exposure.
2	E	,,	,,	,,	Plane $CuSO_4Am$ on $CuSO_4Am$	,,	,,	42 ,,	,,	,,	Sharp E at 9 A.M., 14th, but not all killed.	
3	C	,,	,,	,,	Plane fuchsin on fuchsin	,,	,,	,,	,,	,,	C showing at 9 A.M. on 14th, but by no means all killed.	
4	X	,,	,,	Reflected sun	Bottle $CuSO_4$ + I and CS_2	12.15—2.25 P.M.	,,	6 days	...	,,	Germinating evenly all over, and apparently no effect.	
15	K	Mar. 18	Mar. 18	Direct sun	Bottle $CuSO_4$ + I and CS_2	to 4 P.M.	4.30 P.M., Mar. 18	7 ,,	9 A.M. on 20th	,,	Both letters very faint, and obliterated on 23rd, or nearly so.	Sun hot and bright, blue sky, air cool. Slight haze and glare later.
	Z	,,	,,	,,	Æsculin + quinine	,,	,,	,,				
III	N	Mar. 24	Mar. 25	,,	CS_2 + I on alum	11.30 A.M. to 3.30 P.M.	4 P.M., Mar. 25	...	...	...	Negative	Hot sun, but very hazy. Plates ruined by heat.
IV	O	,,	,,	,,	CS_2 + I on water	,,	,,					
XII	Z	Mar. 27	Mar. 27	,,	CS_2 + I on alum	12 A.M. to 3 P.M.	3 P.M., Mar. 27	3 days	9 A.M. on 28th	25° C.	Extremely faint letters, and only inhibited: obliterated on 29th, except a ghost of K.	Very hot sun, hazy first hour, then brilliant.
XIII	K	,,	,,	,,	CS_2 + I on water	,,	,,	,,				

However, on comparing the action of a thick crystal of rock-salt with that of water and alum, I was unable to detect any such marked difference as would seem to follow if that conclusion were correct. As will be seen more clearly later, when I come to discuss the results obtained with the spectrum, the infra-red rays are themselves utterly without perceptible effect.*

§ VIII.

The following series of experiments, designed to estimate the degree of light action on water bacteria, was carried out, under my direction and supervision, by Miss Hayward, of University College, London, and I owe it to her to state that their successful carrying out would have been impossible—on account of the numerous plates to be counted, in short periods, and involving very large numbers—but for her untiring industry and devotion to the work.

Series I.

About 150 c.c. of Thames water, collected at 10 A.M. on August 12, were distributed equally in three Erlenmeyer flasks, properly sterilised, and the flasks labelled A, B, and C; and at 11 A.M. a 1-drop plate was made from each flask. These plates, examined and counted on August 14 at 11 A.M., gave respectively 1560, 1700, and 1080 colonies per c.c.—*i.e.*, an average of 1446 colonies per c.c. developing in two days. On keeping the plates another day, two of them gave 3705 and 1656 respectively, while the third was uncountable and liquefied, the mean being 2680.

We, therefore, assume that the water contained at the outset about 2700 bacteria per c.c., capable of developing in three days.

The flasks were then placed as follows:—A was suspended by the neck so that it could be exposed to what sunshine there was, and at the same time be illuminated from below by the light reflected from a plane silvered mirror.

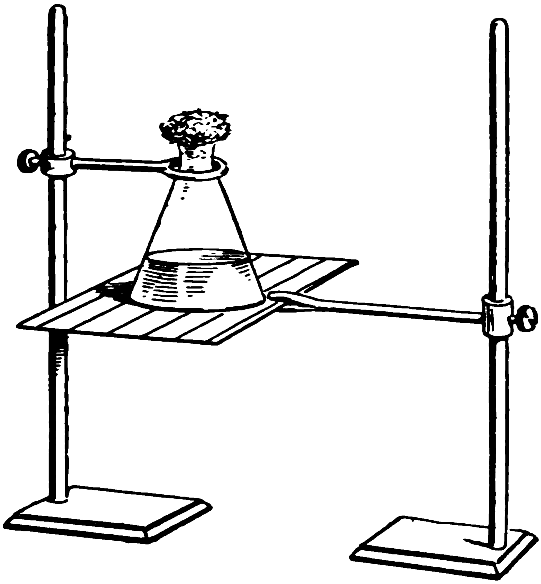
B and C merely stood by the side of the stand supporting A, C being covered with tin-foil and black paper, while B was exposed to the light from above and at the sides.

After standing thus from 12 noon to 4.30 P.M., the weather being very cloudy with only occasional bursts of sunshine, fresh samples were taken from each flask by means of sterilised pipettes, and new plates made to see if any changes had occurred of note.

Taking first the plate from A, after two days' incubation, we found 1599 colonies per c.c.; and after a further twenty-four hours, 2760 per c.c., of which 12 per cent. were liquefying forms. The number of living bacteria capable of developing in two to three days, there-

* Since writing this the experiments with the spectrum have been published separately (See 'Proc. Roy. Soc.,' 1894, vol. 54, p. 472, Abstract).

FIG. 3.



fore, was approximately the same as at the outset, and the dull light seems to have prevented the usual rapid multiplication.

The plate from B, made after exposure, gave us 680 colonies per c.c. after two days' and 2550 colonies per c.c. after three days' incubation, and showed evident signs of liquefaction. Here, therefore, it would seem that the light had exercised an inhibitory action as to numbers.

The plate from C gave 2268 colonies per c.c. after three days' incubation, but it was liquefying still more rapidly.

So far, therefore, with the dull light of a cloudy day, it did not seem as if an exposure of four and a half hours gave results of much significance as to the numbers of bacteria; but it *did* seem as if the plates from the exposed flasks showed less liquefaction.

Meanwhile, the three flasks stood at a temperature of about 16° C. overnight in the laboratory, and were exposed next day from 11.30 A.M. to 4.30 P.M.—again a dull day, and practically no sunshine at all.

Before exposure, however, we took samples as before, and after two days' incubation found that A had about 13,650 per c.c., B had 15,980 per c.c., and C was so badly liquefied that we could place no reliance on the numbers (1584) counted.

These numbers are not very satisfactory taken by themselves, but they showed us that the matter was worth further investigation along similar lines.

The following table H summarises the foregoing facts:—

Table H.

Flask.	Contents.	Exposed or not.	Hours of exposure.	Number of plate.	When made.	When examined.	Number of colonies.	Number of bacteria per c.c.	Remarks.	When water placed in flask.
A	Thames water	Exposed with mirrors	0	A_1	Aug. 12, 11 A.M.	Aug. 14, 11 A.M.	40–95	1560–3705		Aug. 12, 11 A.M.
						Aug. 15, 12.30 P.M.				
	”	”	4½	A_2	Aug. 12, 5 P.M.	Aug. 14, noon	41–70	1599–2760	Over-night in laboratory, 19 hours at 16° C	”
						Aug. 15, 1 P.M.				
	”	”	..	A_3	Aug. 13, 11 A.M.	Aug 15, 4 P.M.	350	13,650		”
B	Thames water	Exposed without mirrors	0	B_1	Aug. 12, 11 A.M.	Aug. 14, 11 A.M.	50	1700		Aug. 12, 11 A.M.
						Aug. 15, 12.30 P.M.	Liquefied	, uncountable		
	”	”	4½	B_2	Aug. 12, 5 P.M.	Aug. 14, noon	20–75	680–2550	Over-night in laboratory, 19 hours at 16° C	”
						Aug. 15, 3 P.M.				
	”	”	..	B_3	Aug. 13, 11 A.M.	Aug. 15, 5 P.M.	470	15,980		”
C	Thames water	Not exposed	0	C_1	Aug. 12, 11 A.M.	Aug. 14, 11 A.M.	30–46	1080–1656		Aug. 12, 11 A.M.
	”	”	0	C_2		Aug 15, 12.30 P.M.				
					Aug. 12, 5 P.M.	Aug. 14, 1 P.M.	35–63	1260–2268	Over-night in laboratory, 19 hours at 16° C	”
	”	”	0	C_3		Aug. 15, 3.30 P.M.				
					Aug. 13, 11.15 A.M.	Aug. 15, 6 P.M.	44*	1584		”

* Actually counted, but badly liquefied, and the number would have been much higher later.

§ IX.

On August 14th, three flasks were prepared and exposed as before; A′ with mirror beneath, B′ with no mirror, and C′ wrapped up. Fairly bright sunshine prevailed during the exposure—from 11.15 to 4.30—an occasional cloud obscuring the sun.

Two samples showed that A′ started with 1755 per c.c. and 1404, the mean being 1578 per c.c. After five hours' exposure, over the mirror, plates were again taken. Three plates yielded 1326, 780, and 858 per c.c., the mean being 988 per c.c., which looks as if a perceptible reduction had occurred.

Two plates from B′ at the start gave 680 and 2176, the mean being 1423 per c.c. After its five hours' exposure, without a mirror, three samples yielded 918, 476, and 476 per c.c., the mean being 623 per c.c., and again suggesting effect of bactericidal rays.

Plates from C′ at the beginning gave 1156 as the number to start with, and after the five hours side by side with the other flasks, but protected from the sunshine by foil and paper, samples gave 3240, 2052, and one uncountable. The mean, = 2646 per c.c., suggesting a perceptible increase.

Here, again, it was evident that liquefaction took place much more rapidly on the plates from the unexposed flask than on those from the flasks exposed to light.

The chief difficulty with these mixed plates is always that caused by the liquefying forms, one of which was especially troublesome, often coming on so rapidly that a plate which looked "safe" at a given time would be ruined three or four hours later.

The foregoing results are summarised in the following Table I.

Without attempting to lay too much stress on the actual numbers in this series, it is pretty evident that if we take the totals or the means of the numbers of bacteria obtained from the water by taking *three* samples from each flask at each period of examination, we get at least some information as to the rate of action of the light on the total organisms.

Put thus, the facts run as follows:—Of the nine samples taken at the start, four were not counted, as they liquefied too rapidly. The average of the other five gave 1434 per c.c.

Flask A′, after five hours' exposure over a mirror, gave 988 per c.c. as the mean of three samples.

Flask B′, after five hours' exposure without a mirror, gave 623 per c.c. as the mean of three plates.

Flask C′, not exposed, but otherwise treated similarly, gave 2646 as the mean of two plates.

It seems impossible to doubt, therefore, that the exposure to light reduced the numbers by nearly one-half. But this proportion becomes

much greater if we note that during the period an enormous multiplication would normally occur.

§ X.

On August 15th two Erlenmeyer flasks were charged as before with Thames water, and labelled A and C. A was exposed over mirrors, and C wrapped up. We introduced the difference here of having an additional mirror *behind* the flask, as well as that below.

The day was bright, with plenty of sunshine all the time, and A was exposed for 5½ hours—from 10.30 a.m. to 4 p.m.—and then samples taken from both.

Meanwhile, the average of seven samples taken at the commencement gave 1644 as the number per 1 c.c. in the Thames water at starting.

Unfortunately, the temperature rose during the next twenty-four hours sufficiently to soften the gelatine of the plates taken after the first 5½ hours, so we could not count these.

On August 16th—another bright, clear day—the flask A was again exposed for six hours, and C (wrapped up) beside it, both flasks having stood all night (nearly 12 hours), at 18° C in a cupboard in the laboratory.

After this second exposure the plates gave—for A about 6000 per c.c., and for C over 174,000 per c.c.; showing that the exposure to the light had kept down the numbers in A, in spite of the interval of twelve hours in a warm, dark cupboard, when, of course, the bacteria not killed off by the first day's exposure multiplied rapidly.

Here, again, I was struck by the diminution of the liquefaction on the plates from the flask A; it did not look like merely fewer liquefying forms, but as if those that were present really liquefied less rapidly and less efficiently than those from the flask not exposed to light.

§ XI.

On August 22nd two Erlenmeyer flasks, marked F 3 and F 4, were charged to a depth of 1 in. with Thames water, properly collected, &c. (see Table J).

Flask F 3 was exposed to the sun with a mirror below; F 4 stood, covered, by its side. The exposure lasted from 11.30 A.M. to 4.30 P.M. (being five hours), but only about one and a half to two hours at most was good sunlight, the day being showery with snatches of blue sky at intervals.

Four samples taken at the beginning of the experiment gave 800, 1408, 748, and 1462 per c.c. as the numbers after two days' incubation. Total of four plates = 4418; average = 1104 colonies per c.c. to start with.

Table I.

Flask.	Contents.	Hour of collection.	Exposed or not.	Hours of exposure.	Number of plate.	When made.	When examined.	Incubation.	Number of colonies.	Number of bacteria per c.c.
A′	Thames water	Aug. 14, 10.30 A.M.	Exposed with mirror	0	A 1′	Aug. 14, 11 A.M.	Aug. 16, 10 A.M.	Hours. 47	45	1755
	„	„	„	5	A 2′	Aug. 14, 5 P.M.	Aug. 16, 11.40 A.M.	42½	34	1326
A′	Thames water	Aug. 14, 10.30 A.M.	Exposed with mirror	0	A 1′	Aug. 14, 11 A.M.	..	..	Liquefied and uncountable	
	„	„	„	5	A 2″	Aug. 14, 5 P.M.	Aug. 16, 2.20 P.M.	46½	20	780
A′	Thames water	Aug. 14, 10.30 A.M.	Exposed with mirror	0	A 1‴	Aug. 14, 11 A.M.	Aug. 16, 10 A.M.	47	36	1404
	„	„	„	5	A 2‴	Aug. 14, 5 P.M.	Aug. 16, 2.20 P.M.	46½	22	858
B′	Thames water	Aug. 14, 10.30 A.M.	Exposed without mirror	0	B 1′	Aug. 14, 11 A.M.	Aug. 16, 11 A.M.	48	20	680
	„	„	„	5	B 2′	Aug. 14, 5 P.M.	Aug. 16, 12 noon	45	27	918

B′	Thames water	Aug. 14, 10.30 A.M.	Exposed without mirror	0	B 1″	Aug. 14, 11 A.M.	Aug. 16, 2 P.M.	Hours. 51	Liquefied and uncountable	
	”	”	”	5	B 2″	Aug. 14, 5 P.M.	Aug. 16, 3 P.M.	46	14	476
B′	Thames water	Aug. 14, 10.30 A.M.	Exposed without mirror	0	B 1‴	Aug. 14, 11 A.M.	Aug. 16, 1 P.M.	50	64	2176
	”	”	”	5	B 2‴	Aug. 14, 5 P.M.	Aug. 16, 3 P.M.	46	14	476
C′	Thames water	Aug. 14, 10.30 A.M.	Not	..	C 1′	Aug. 14, 11.30 A.M.	Aug. 16, 11 A.M.	..	Liquefied and uncountable	
	”	”	”	..	C 2′	Aug. 14, 5 P.M.	Aug. 16, 12.30 P.M.	..	90	3240
C′	Thames water	Aug. 14, 10.30 A.M.	..	..	C 1″	Aug. 14, 11.30 A.M.	Aug. 16, 11 A.M.	..	34	1156
	”	”	Not	..	C 2″	Aug. 14, 5 P.M.	Aug. 16, 5.20 P.M.	..	Liquefied and uncountable	
C′	Thames water	Aug. 14, 10.30 A.M.	..	..	C 1″	Aug. 14, 11.30 A.M.	..	..	Liquefied and uncountable	
	”	”	Not	..	C 2‴	Aug. 14, 5 P.M.	Aug. 16, 5.20 P.M.	..	57	2052

After another day's incubation one plate was liquefied; the others gave 832, 1920, and 816. Total of three plates = 3568; average = 1189 colonies per c.c., and this may be taken as the highest number obtainable, the counting having been done and checked very carefully and thoroughly with a good lens.

After the five hours' exposure, new plates were made—two from each of the flasks—and, in order to obviate as far as possible our previous difficulties with the liquefying forms, we diluted each 1 c.c. of the sample water with 9 c.c. of sterile distilled water, carefully prepared in advance.

It is unnecessary to give details as to sterilisation, &c., but the following are the essential points of the plan followed: For *each* sample to be taken two pipettes and two test-tubes are needed. One of the test-tubes of each pair is graduated to hold 9 c.c. of the sterile water; into the other 1 c.c. of the water to be tested is dropped with one pipette, and the 9 c.c. of sterile water are then poured on to this, thus ensuring thorough and rapid mixture. The second pipette is then used to obtain the one or more drops taken to make the gelatine plate.

By this method we found that two plates from F 3 (the exposed flask) gave 2880 and 1280 per c.c. on the 4th day. Total of two plates, 4160; mean, 2080 per c.c., suggesting that the light was not strong enough to prevent the bacteria from multiplying. On standing two days longer these plates gave 3840 and 1600; total, 5440; mean, 2720 colonies per c.c., showing that there were a good many slowly developing germs present, and we were struck with the paucity of liquefying forms.

Two plates from F 4 (the unexposed flask), made and examined at corresponding times, and in the same way, gave 660 and 2310 per c.c. on the fourth day. Total, 2970; mean, 1485 colonies per c.c.; and on the fifth day (we could not go further) 660 and 3630; total, 4290; mean, 2145 colonies per c.c.

So far, therefore, the numbers in the two flasks did not appear to be appreciably affected by the little sunlight that reached the water.

The two flasks were now placed in an ice-safe over night, for exposure next day. They remained on ice about 14 hours.

Next morning, August 23, these flasks were again put out at 9 A.M., and remained till about 4 P.M. (over seven hours); but it rained steadily all the time except the last hour, when the sun shone.

Two plates, made of diluted samples as before, from each flask, were made at noon on this day, with the following results: After two days' incubation the plates from flask F 3 (exposed) showed 39,600 and 37,620 per c.c. Total of two plates, 77,220; mean, 38,610. After a further day's incubation the plates gave 47,190 and 41 250; total, 88,770; mean, 44,385 per c.c.

The two plates from flask F 4 (not exposed), examined at the same time, gave 10,800 and 7,200 per c.c. in two days. Total, 18,000; mean, 9000 per c.c.; and, in three days, they showed 13,200 and an uncountable number, owing to liquefaction.

These results were decidedly mystifying at first, for they showed apparently a stimulating effect of exposure to the light; but on going further into the matter it seems more probable that what really happens is, that (1) the sunlight was not powerful enough in blue-violet rays to produce any appreciable inhibition in the time; and (2) the flask F 4, covered in tin foil, &c., did not become warmed so rapidly as the other, and consequently still showed the retarding action of the icing to which it had been subjected.

That this explanation is right is borne out by the behaviour of the plates taken at 4 P.M.—*i.e.*, after four hours' further exposure of the flasks—for the covered flask, although enormously increased in bacteria, was still behind the exposed one.

After two days' incubation, the two plates from F 3 (exposed) at this period gave 184,800 and 165,000 respectively. Total, 349,800; mean, 179,900 per c.c.; and, after three days, 231,000 and 207,900. Total, 438,900; mean, 219,450 per c.c.

Whereas the two plates from the non-exposed flask gave respectively 19,500 and 12,000 per c.c.; total, 31,500; mean, 15,750 per c.c., after two days' incubation, and were not counted further.

Considering the enormous expenditure of time and trouble involved in making and counting these plates, we were somewhat discouraged by these negative results, which are summarised in the accompanying Table J (p. 346, &c.).

§ XII.

On August 24th two flasks of Thames water labelled F 5 and F 6 were exposed exactly as the last, but the weather was fine and we had much bright sun and blue sky, with rapidly moving white clouds.

The first exposure was from 9.30 A.M. to 4 P.M., and the arrangement as before. The temperature, as indicated by thermometers in control flasks, rose occasionally over 35° C, but was usually not above 30° C, and somewhat higher in the covered flask than in the uncovered one.

Four plates of the water with which the flasks were charged were made at the time of starting the experiment, and were incubated as long as possible. In four days they gave us 1980, 1650, 1320, and 2970 colonies per c.c. Total of the four samples, 7920; average, 1980 colonies per c.c.

After six and a half hours' exposure, of which about four hours was brilliant sunshine as far as we could estimate, two plates from each

Table J.

Flask.	Contents.	When filled.	Exposed or not.	Time of exposure.	Number of hours exposed.	Number of hours of sun.	Other treatment.	Number of plate.	When made.
F 3	Thames water (collected Aug. 22, 10 A.M.) ½ in. deep	Aug. 22, 11.30 A.M.	Not	..	0	0	..	P 17	Aug. 22, 11.30 A.M.
"	"	"	"	..	0	0	..	P 18	"
F 4	"	"	"	..	0	0	..	P 19	"
"	"	"	"	..	0	0	..	P 20	"
F 3	"	"	Exposed mirror below	Aug. 22, 11.30 A.M.—4.30 P.M.	5	1½—2	..	P 23	Aug. 22, 5 P.M.
"	"	"	"	"	5	1½—2	..	P 24	"
F 4	"	"	Not	..	0	0	..	P 25	"
"	"	"	"	..	0	0	..	P 26	"
F 3	"	"	Exposed mirror below	Aug. 22, 11.30 A.M.—4.30 P.M. Aug. 23 9 A.M.—12 noon & 12—4.30	7½	1½—2	Flask left at laboratory, temperature 16—18° over-night	P 27	Aug. 23, 12 noon.
"	"	"	"	"	7½	1½—2	"	P 28	"

Table J.

When examined.	Hours of incubation.	Temperature of incubation.	Number of colonies on plate.	Number of bacteria per c.c.	Remarks.	Weather.
(1) Aug. 24, 1 P.M.	49½	16—18°	25	800	7 small liquefying colonies.	
(2) Aug. 25, 3 P.M.	75½	„	26	832	10 liquefying colonies.	
(1) Aug. 24, 2.30 P.M.	51	„	44	1,408	6 liquefying.	
(2) Aug. 25, 3 P.M.	75½	„	60	1,920	14 liquefying.	
(1) Aug. 24, 3 P.M.	51½	„	22	748	6 liquefying.	
(2) Aug. 25, 3 P.M.	75½	„	24	816	9 liquefying.	
(1) Aug. 24, 3 P.M.	51½	„	43	1,462	11 liquefying.	
(2) Aug. 25, 3 P.M.	75½	„	liquefied and therefore uncountable			
(1) Aug. 26, 9 A.M.	88	„	9	2,880	1 c.c. diluted to 10 with sterile distilled water, and 1 drop of the mixture taken. None liquefying. 1 mould present additionally	Heavy clouds and showers, with blue sky and sunshine between.
(2) Aug. 27, 12.30 P.M.	115½	„	12	3,840	Diluted. 1 mould	Ditto.
(3) Aug. 28, 12.30 P.M.	139½	„	..	..	2 small liquefying. 2 moulds	Ditto.
(1) Aug. 26, 9 A.M.	88	„	4	1,280	Diluted. None liquefying. 1 mould	Ditto.
(2) Aug. 27, 12.30 P.M.	115½	„	5	1,600	Ditto	Ditto.
(3) Aug. 28, 12.30 P.M.	139½	„	..	..	1 slow liquefier appeared.	Ditto.
(1) Aug. 26, 9.30 A.M.	88½	„	2	660	Diluted. No liquefying Plate on ice till next morning 9 A.M.	
(2) Aug. 27, 1 P.M.	115½	„	2	660	2 moulds appeared.	
(1) Aug. 26, 9.30 A.M.	88½	„	7	2,310	No liquefying.	
(2) Aug. 27, 1 P.M.	115½	„	11	3,630	No liquefying.	
(1) Aug. 26, 10 A.M.	70	„	120	39,600	Diluted. 4 small liquefiers	Steady rain all the morning; an hour's sun 3—4 P.M., but feeble.
(2) Aug. 27, 4 P.M.	100	„	143	47,190	9 liquefiers. 1 mould appeared	
(3) Aug. 28, 12.30 P.M.	120½	„	..	..	11 good sized liquefiers, and many small ones	
(1) Aug. 26, 10 A.M.	70	„	114	37,620	Diluted. 5 liquefying, all small	Ditto.
(2) Aug. 27, 4.30 P.M.	100½	„	125	41,250	Diluted. 16 liquefying	Ditto.

Table J—

Flask.	Contents.	When filled.	Exposed or not.	Time of exposure.	Number of hours exposed.	Number of hours of sun.	Other treatment.	Number of plate.	When made.
F 4	Thames water (collected Aug. 22, 10 A.M.) ½ in. deep	Aug. 22, 11.30 A.M.	Not	..	0	0	Flask left at laboratory, temperature 16—18° over-night.	P 29	Aug. 23, 12.30 P.M.
,,	,,	,,	,,	..	0	0	,,	P 30	,,
F 3	,,	,,	Exposed	Aug. 22, 11.30—4.30 Aug. 23, 9—12 12—4.30	12½	2½—3	,,	P 31	Aug. 23, 4.30 P.M.
,,	,,	,,	,,	,,	12½	2½—3	,,	P 32	,,
F 4	,,	,,	Not	..	0	0	,,	P 33	Aug. 23, 5 P.M.
,,	,,	,,	,,	..	0	0	,,	P 34	,,

flask were made, and incubated also as carefully and long as possible, to get the maximum numbers.

The two plates from the exposed flask F 5 gave 160 and 320 after four days' incubation. Total, 480; mean, 240 per c.c.; whereas those from the unexposed flask gave 3400 and 5916 respectively in the same period. Total, 9316; mean, 4658 colonies per c.c.

These numbers show very distinctly the effect of the sunshine, and are borne out clearly by what follows.

After the exposure on August 24th, and after the samples for plates had been taken, both flasks were placed on ice over-night, and remained on ice about fourteen hours.

On the 25th, at 9.30 A.M., the exposure was repeated, the flasks being first examined as to the effects of their sojourn in the ice-box. The numbers were found to have remained remarkably constant, being 210 for the exposed and 3136 for the covered flask.

continued.

When examined.	Hours of incubation.	Temperature of incubation.	Number of colonies on plate.	Number of bacteria per c.c.	Remarks.	Weather.
(1) Aug. 26, 10 A.M.	69½	16—18°	32	10,800	Diluted. 8 liquefying.	
(2) Aug. 27, 4.30 P.M.	100	"	44	13,200	" "	
(1) Aug. 26, 11 A.M.	68½	"	24	7,200	" 3 liquefying.	
(2) Aug. 27, 4 P.M.	99½	"	liquefied and therefore uncountable			
(1) Aug. 26, 10.30 A.M.	66	"	560	184,800	Diluted. 15 liquefying	Steady rain all the morning; an hour's sun 3—4 P.M., but feeble.
(2) Aug. 27, 5 P.M.	96½	"	700	231,000	Diluted. About 55 liquefying	Ditto.
(1) Aug. 26, 11 A.M.	66½	"	500	165,000	7 large and many small liquefying.	Ditto.
(2) Aug. 27, 5 P.M.	96½	"	630	207,900	Diluted	
Aug. 28, 11.30 A.M.	66½	"	65	19,500	Diluted. 6 liquefying. 1 mould.	
"	"	"	40	12,000	Diluted. 1 liquefying.	

After this day's exposure, which lasted six hours, about half of which was more or less cloudy at intervals and the rest bright sunshine, the plates made again showed great differences, clearly pointing to the inhibitory action of the sunlight, for the sample of exposed water gave 1584 as against 29,760 per c.c. in the unexposed flask.

In other words, although flask F 5 had stood fourteen hours in the dark at a temperature not too low for increase, its exposure of about 12½ hours to the light had resulted in the reduction of its bacteria to a number below that it started with; the unexposed flask meanwhile had its bacteria multiplying normally at the rapid rates usual for these waters. Had the sunshine been more intense during this second day, it is by no means improbable that the water could have been completely sterilised by exposure.

The results discussed above are put into a tabular form in the following Table K:—

Table

Flask.	Contents.	When filled.	Exposed or not.	Time of exposure.	Hours of exposure.	Hours of sun.	Other treatment.	Plate.	When made.
F 6	Thames water 1 in. deep. Collected Aug. 24, 9.10 A.M.	Aug. 24, 9.30 A.M.	Not	..	0	0	..	P 37	Aug. 24, 10 A.M.
"	"	"	"	..	0	0	..	P 38	"
"	"	"	"	..	0	0	..	P 39	"
"	"	"	"	..	0	0	..	P 40	"
F 5	"	"	Exposed with mirror	Aug. 24, 9.30 A.M.—4 P.M.	6½	4	..	P 39*a*	Aug. 24, 5 P.M.
"	"	"	"	"	6½	4	..	P 40*a*	"
F 6	"	"	Not	..	0	0	..	P 41	"
"	"	"	"	..	0	0	..	P 42	"
F 5	"	"	Exposed with mirror	Aug. 24, 9.30 A.M.—4 P.M.	6½	4	After having been placed on ice overnight from 5.30 P.M.—9.30 A.M.	P 43	Aug. 25, 9.30 A.M.
F 6	"	"	Not	..	0	0	"	P 44	Aug. 25, 10 A.M.
F 5	"	"	Exposed with mirror below	Aug. 24, 9.30 A.M.—4 P.M. Aug. 25, 9.30 A.M.—10.30 P.M.	12½	7½	..	P 49	Aug. 25, 3.30 P.M.
F 6	"	"	Not	..	0	0	..	P 50	"

§ XIII.

On August 25 two flasks labelled F 7 and F 8 were exposed as the last, from 9.30 A.M. to 4 P.M., weather mixed—clouds and bright sunny intervals.

Four samples taken at the beginning gave 1666, 1292, 1394, and 1768 colonies per c.c.; total, 6120; average, 1530 germs per c.c. to start with.

K.

When examined.	Hours of incubation.	Temperature of incubation.	Number of colonies on plate.	Number of bacteria per c.c.	Remarks.	Weather.
Aug. 28, 12 noon	98	16—18°	6	1,980	Diluted. 3 moulds also present.	
"	"	"	5	1,650	Diluted. 2 moulds also present.	
"	"	"	4	1,320	Diluted. 1 grey liquefier. 1 mould also.	
"	"	"	9	2,970	Diluted. 2 liquefying.	
Aug. 28, 12.30 P.M.	91½	"	5	160	No dilution. 5 moulds also present	Rapidly moving clouds, blue sky, and bright sun.
"	"	"	10	320	3 moulds also present	"
(1) Aug. 27, 5 P.M.	72	"	90	3,060	11 liquefying.	
(2) Aug. 28, 12.30 P.M.	91½	"	100	3,400	Very much liquefied.	
"	91½	"	174	5,916	9 liquefying. 1 small mould also.	
Aug. 28, 12.30 P.M.	75	"	7	210	None liquefying.	
Aug. 28, 1 P.M.	75	"	98	3,136	6 liquefying.	
Aug. 28, 4.30 P.M.	73	"	48	1,584	3 liquefying and 1 mould also	Clouds and bright sunshine.
"	73	"	930	29,760	About 27 liquefying.	

After the six and a half hours' exposure two plates were made from each flask. The two from F 7 (exposed) gave 93 and 31 per c.c.; total, 124; mean, 62 colonies per c.c.; while those from F 8 (covered) in the same period of incubation, viz., four days, gave 2190 and 2130; total, 4320; mean, 2160 per c.c.

Here again, therefore, the direct sunshine has a decided bactericidal effect, as is clear from details in the following Table L:—

Table

Flask.	Contents.	When filled.	Exposed or not.	Time of exposure.	Hours of exposure.	Hours of sun.	Plate.	When made.
F 8	Thames water 1 in. deep. Collected Aug. 25, 9.10 A.M.	Aug. 25, 9.30 A.M.	Not	..	0	0	P 45	Aug. 25, 10.30 A.M.
"	"	"	"	..	0	0	P 46	"
"	"	"	"	..	0	0	P 47	"
"	"	"	"	..	0	0	P 48	"
F 7	"	"	Exposed mirror below and behind	Aug. 25, 9.30 A.M. – 4 P.M.	6½	4	P 51	Aug. 25, 4 P.M.
"	"	"	"	"	6½	4	P 52*a*	"
F 8	"	"	Not	..	0	0	P 53*a*	"
"	"	"	"	..	0	0	P 54*a*	"

§ XIV.

On August 28 another pair of flasks (F 9 and F 10) were exposed as before, 10.30 A.M. till 6.80 P.M., and left out all night; exposed next day from 10 A.M. to 4 P.M. The sky was cloudy on both days, but there was a good deal of sunshine also, particularly on August 29. We estimated the actual exposure to daylight as 18 hours, and about 8 hours' good sun altogether.

In order to extend our numbers whence we drew the average of bacteria present at the commencement, *twelve* samples were taken for plates to begin with, and we diluted to 1 in 10 as before.

After three days' incubation we found four of the plates too far liquefied to count, but the numbers would probably not be inconsistent with the following:—

The eight plates gave a total of 10,890 colonies, and an average of 1361 colonies per c.c. in the water at the commencement of the experiment.

After the exposure to light and darkness from 10.30 A.M. August 28 to 4 P.M. August 29, we found about 300 per c.c. in the flask ex-

L.

When examined.	Hours of incubation.	Temperature of incubation.	Number of colonies on plate.	Number of bacteria per c.c.	Remarks.	Weather.
Aug. 28, 3.30 P.M.	77	..	49	1666	6 liquefying; also 1 mould appeared.	
Aug. 30, 12 noon	121½	..	100	3400		
Aug. 28, 4 P.M.	77½	..	38	1292	6 liquefying.	
Aug. 30, 12 noon	122	..	64	2716		
Aug. 28, 4 P.M.	77½	..	41	1394	5 liquefying; also 1 mould.	
Aug. 30, 11.30 A.M.	121½	..	79	2686		
Aug. 28, 4 P.M.	77½	..	52	1768	9 liquefying.	
Aug. 30, 10.30	119½	..	136	4624		
Aug. 29, 11.30 A.M.	91½	..	3	93	1 mould also.........	Clouds and bright sunshine.
"	91½	..	1	31		
(1) Aug. 28, 4.30 P.M.	72½	..	58	1740	5 liquefying.	
(2) Aug. 29, 11 A.M.	91	..	73	2190	6 liquefying; also 1 mould.	
Aug. 29, 11.30 A.M.	91½	..	71	2130	5 liquefying.	

posed to the light, and numbers too high to count in that covered with foil and paper; moreover, the latter plates were so badly liquefying that their marked contrast to the former could not escape observation.

It was clear that exposure to sunlight affects the liquefying powers of the forms in the Thames water, and this apart either from the difference in numbers on the plates or because it eliminates these forms more rapidly.

§ XV.

On August 29 two flasks were prepared as before. F 11 was exposed to sun with mirrors, &c., and F 12 put by its side covered with foil and black paper.

The first exposure was from 9.30 A.M. to 12.30 A.M.—three hours' good sunshine, though with occasional clouds.

The water to start with was estimated to contain about 1200 germs per c.c.

After three hours' exposure F 11 gave, as the result of two plates,

99 and 132; total = 231; mean = 115 per c.c. After the same time F 12 (unexposed) gave 1600 and 1920; total, 3520; mean, 1760.

The flasks were meanwhile put back and exposed yet another 4½ hours to the afternoon sunshine—*i.e.*, from 12.30 to 5 P.M.—of which about 3½ hours counted as bright sunshine.

Two plates from F 11 made at 5.30 gave 248 and 279; total, 527; mean, 268 colonies as the number per c.c.; while two plates made at the same time from the unexposed flask (F 12) were so badly liquefied that, although we estimated 3300 per c.c. from one of them, we regard the numbers as really higher.

These flasks stood in the laboratory over-night at a temperature of 18° C., and were then exposed next day from 11.30 A.M. to 4.30 P.M., about one and a half or two hours of the five being sunny. Then, at 5 P.M., fresh plates were prepared.

Two plates from the exposed flask gave 640 and 3200 per c.c. respectively; total, 3840; mean, 1920 per c.c. The numbers are not very good, as there is such a great difference between the two plates.

Two plates from the unexposed flask gave 12,800 and 16,000 per c.c.; total, 28,800; mean, 14,400 per c.c., again bearing out the conclusion that the sun has powerful action on the exposed water.

§ XVI.

To my mind one of the most important discoveries elicited from these plate cultures of Thames water was the obvious reduction of liquefaction on the plates made from water exposed to light, and so struck was I with the differences between these plates and those made with the water not exposed that I made an independent investigation into the matter by selecting a set of the most pronounced liquefying forms from the water and examining their behaviour when exposed to light side by side with that of non-exposed samples.

I started with the commonest and most pronounced liquefying form in the Thames water at the time. I refer to it throughout as Colony β in my notes, and write it shortly β. I had noticed the following facts concerning it during our experiments on the action of light on the Thames water as collected—it should be borne in mind that I had already studied its characters and knew the form pretty well.

In the first place the plates as a whole liquefied very much more slowly and less completely than those made from unexposed water. Secondly, although it seemed at times as if this was because the form β had been eliminated from the water, I suspected that a certain other form, which liquefied less rapidly and developed much more slowly altogether, was really the above-mentioned form β with feebler characteristics.

On September 3 a small loopful of a gelatine culture of the bacillus β was carefully shaken in about 50 c.c. of sterile water, and distributed equally in two Erlenmeyer flasks labelled β (1) and β (2).

Flask β (1) was wrapped in foil and black paper; β (2) was exposed over mirror from 9.30 A.M. to 5 P.M., and about five hours of these seven and a half were good strong sunshine. Thermometers in control flasks went up to 34—35° C. as the highest temperature registered in the afternoon.

At 9.30 a plate was prepared from each flask, and gave something like 3,000,000 per c.c. as the average numbers to start with.

After exposure, two plates were prepared from each. Of the two plates from the exposed flask, one plate—a 1-drop plate of a 1/10th dilution—yielded no colonies at all; the other gave 21,941 per c.c., pointing to a profound light-action.

Of the two plates from the darkened flask, one gave 5,400,000, and the other 5,000,000 as the nearest estimate per c.c.

The two flasks meanwhile stood over-night in the laboratory at 18° C. for about fourteen hours, and at 7 A.M. next day (September 4) I made two plates from the exposed flask and then again put them out as before.

These two plates gave 42,000 and 59,220 respectively; total, 101,220; mean, 50,610 bacteria per c.c., a prefectly natural rise in the numbers having occurred during the night.

No plates were taken from the other flask, as I had no particular need for the numbers—known to be very high—and wished to reserve the counting for other plates.

After exposure to the bright sunshine of September 4, from 8 to 4.30, say eight hours' sunshine on the exposed flask, two new plates were made from each flask.

The two plates from exposed flask gave 3050 and 4200; total, 7250; mean, 3625 bacteria per c.c., again showing a marked reduction in the sunlight, and the plates were singularly free of liquefying centres, but showed many colonies of the kind I had previously suspected as being the representatives of β.

Of the two plates from the unexposed flask, although made from one drop each of a 1/20th dilution, the numbers were again so large and the liquefaction so rapid that no reliance can be placed on them, except that they prove that no essential diminution was to be traced to the action of the water or temperature in the absence of light, but only the natural fall in numbers always found when water stands for some time. The numbers actually calculated were 660,000 and 3,300,000; total, 3,960,000; mean, 1,980,000 per c.c.

The flasks stood in the laboratory at 18° C. through the night, and I repeated the exposure on September 5, putting the uncovered flask into the bright sunlight of that day *without* a mirror. It received a

Table M.—Experiments on Insolation.

Flask.	When filled.	Exposed or not.	Time of exposure.	Hours of exposure.	Hours of sun.	Other treatment.	Plate.
β (1)	Sept. 3, at 9 A.M.	Not	..	..	..	..	P. β (1)
β (2)	”	..	..	..	..	..	P. β (2)
β (1)	”	..	..	..	..	..	Pa. β (1)
β (1)	”	..	..	..	..	..	Pb. β (1)
β (2)	”	Exposed	9.30 to 5 on Sep. 3.	7½	5	Stood above and in front of mirror	P^{i} β (2)
β (2)	”	”	”	”	”	”	P^{ii} β (2)
β (2)	”	”	”	”	”	Ditto. Then = 14 hours in dark at 18°C.	P^{iii} β (2)
β (2)	”	”	”	”	”	”	P^{iv} β (2)
β (2)	”	”	9.30 to 5 on Sep. 3 and 8 to 4.30 Sep. 4	16	13	”	P^{v} β (2)
β (2)	”	”	”	”	”	”	P^{vi} β (2)
β (1)	”	Not	..	..	..	In dark whole time = 40 hours	Pc β (1)
β (1)	”	”	..	..	..		Pd β (1)
β (1)	”	”	..	..	..	In dark 64 hours	Pe β (1)
β (2)	”	Exposed	9.30 to 5 on Sep. 3, 8 to 4.30 Sep. 4, 10 to 5 Sep. 5.	23	19	In dark at 18° each night	P^{vii} β (2)

good six hours' sun direct, and the temperature rose as high as 37° C. at one time, but was for the most part at 30—32° C. All the other conditions were as before.

After exposure, a plate from the insolated flask gave 665 per c.c. One from the covered flask gave 2,600,000 as the nearest estimate I could form.

It is obvious, therefore, that the bacterium referred to as Colony β is very sensitive to the solar action, and the results obtained with the above pure cultures are summarised in Table M.

§ XVII.

The results with a second badly liquefying form, which I call

of Bacillus β in Sterile Water.

When made.	When examined.	Time of incubation.	Temperature of incubation.	Dilution or not.	Quantity used.	No. of colonies counted on plate.	Number of bacteria per c.c.
		days.	C°.		c.c.		
9.30 A.M., Sep. 3	Sep. 8	5	16—18	$\frac{1}{20}$	$\frac{1}{30}$	About 5,000	3,000,000
..	..	..	..	$\frac{1}{20}$	$\frac{1}{30}$	5,844 actually counted	3,506,400
5 P.M., Sep. 3	..	..	..	$\frac{1}{10}$	$\frac{1}{36}$	30 squares averaged 500 per sq. = 15,000	5,400,000
..	..	..	..	$\frac{1}{20}$	$\frac{1}{25}$	Very similar numbers	5,000,000
..	..	..	.	$\frac{1}{10}$	$\frac{1}{36}$	0	
..	..	..	..	0	$\frac{1}{37}$	593	21,941
7 A.M., Sep. 4	Sep. 15	11	..	$\frac{1}{20}$	$\frac{1}{21}$	100	42,000
..	..	..	..	$\frac{1}{20}$	$\frac{1}{21}$	141	59.220
4.30 P.M., Sep. 4	..	..	..	0	$\frac{1}{25}$	122	3,050
..	..	..	..	$\frac{1}{10}$	$\frac{1}{35}$	12	4,200
..	..	..	..	$\frac{1}{20}$	$\frac{1}{33}$	About 1,000	660,000
..	..	..	..	$\frac{1}{20}$	$\frac{1}{33}$	About 5,000	3,300,000
5 P.M., Sep. 5	..	10	..	$\frac{1}{20}$	$\frac{1}{26}$	About 5,000	2,600,000
..	..	..	..	$\frac{1}{19}$	$\frac{1}{35}$	1	665

Bacillus η, are not contradictory of the foregoing—indeed they support them so far as they go—but are less conclusive in detail.

On September 5 two Erlenmeyer flasks charged with sterile distilled water, to which a loopful of the bacillus in question was added, were placed out in the usual way. Flask labelled η (1) was covered; flask η (2) was exposed over and in front of plane mirrors.

A plate from each flask at the beginning gave respectively 1,470,144 and 1,699,360; total, 3,169,504; mean, 1,584,752, as the number of bacteria per c.c. to start with.

The flasks were out from 10 A.M. to 4.30 P.M., the day being beautifully bright with a hot sun and blue sky. The temperature in the covered flask rose to 33° C. in the afternoon, that in the exposed

one to about 34° C., as shown by controls with thermometers in the water.

Two samples of the water of the unexposed flask, taken at 4.30 P.M. on September 5, gave 1,575,860 and 1,285,388 as the numbers per c.c.; total, 2,861,248; mean, 1,430,624 per c.c., suggesting that the sojourn in distilled water at that temperature, even in the dark, causes the death of large numbers of this bacillus.

Of two samples taken at 4.30 from the flask η (2), which had been exposed for six and a half hours, neither plate gave any sign of life after ten days' incubation, whence we may assume that neither sample contained a living germ.

These two flasks were put in a cool cupboard over-night—temperature = 15° C.—and again put out on the 6th September from 9.30 to 4.30, so that the exposed flask—η (2)—received another good six hours of bright sun, for the day was brilliantly fine again.

After the exposure, a plate was made from each flask. That from η (2), the one exposed to the sun, gave no signs of life though incubated for ten days; the other showed 560,000 per c.c.

It seems probable, therefore, that in the case of Bacillus η the immersion in sterile water at 33—34° C., even in the dark, is more or less fatal, for we see the bacteria are reduced from over a million and a half per c.c. to nearly half a million per c.c. At the same time it seems pretty clear that when exposed to light at the same time the mortality of the bacilli is much greater. The inference appears fair, but there is naturally some dissatisfaction to be felt with these negative results.

The following table N summarises these facts :—

Table N.—Experiments on Insolation

Flask.	When filled.	Exposed or not.	Time of exposure.	Hours of exposure.	Hours of sun.	Other treatment.	Plate.
η (1)	Sept. 5, 10 A.M.	Not	..	..	..	..	η (1)
η (2)	,,	,,	..	..	..	..	η (2)
η (1)	,,	,,	..	..	..	Covered in the open all day	$\alpha\ \eta$ (1)
η (1)	,,	,,	..	..	..	,,	$\beta\ \eta$ (1)
η (2)	,,	Exposed	10 to 4.30 on Sept. 5	6½	6	Over and in front of plane mirror	I η (2)
η (2)	,,	,,	,,	..	,,	,,	II η (2)
η (1)	,,	Not	..	..	..	Stood over-night at 15°	$c\ \eta$ (1)
η (2)	,,	Exposed	10 to 4.30 Sept. 5, and 9.30 to 4.30 Sept. 6	7	6	,,	III η (2)

* There were 32 squares, averaging about 700

§ XVIII.

On August 20, two Erlenmeyer flasks, labelled F_1 and F_2, were charged to a depth of about an inch with sterile-distilled water with which a loopful of spores of *B. anthracis* had been thoroughly shaken up. Flask F_1 was exposed with a mirror below and one behind; F_2 was wrapped in foil and black paper.

The exposure was from 11.30 A.M. to 5 P.M., but it was a windy and cloudy day, with a good deal of rain. Just before exposure, two plates were made to determine the number of spores introduced per c.c. in the flask F_1. One plate gave 1,950,000, and the other 2,445,000; total, 4,395,000; mean, 2,197,500 per c.c.

Two plates from F_2 gave respectively 2,052,000 and 2,280,000; total, 4,332,000; mean, 2,166,000.

Or, if we take the average of the four plates, we get total = 8,727,000; average of the four = 2,181,750: and it will be noticed how well the four plates agreed.

At 5.30 P.M. two plates were made from the covered flask, and gave per c.c. 1,700,000 and 340,000 respectively. It was noted, however, that the second plate had been badly levelled, and the colonies were heaped up to one side, and could not be properly estimated. Taking the numbers as they stand, we get total = 2,040,000; mean = 1,020,000 per c.c., indicating some reduction, but still enormously high numbers present.

Two plates from the exposed flask F_2, after the five and a half hours' insolation, gave 595,000 and 1,295,000; total, 1,890,000; mean, 945,000.

of Bacillus η in Sterile Water.

When made.	When examined.	Time of incubation.	Temperature of incubation.	Dilution or not.	Quantity used.	No. of colonies on plate.	No of bacteria per c.c.
		Days.			c.c.		
Sept. 5, 10 A.M.	Sept. 8	3	15—18°	$\frac{1}{10}$	$\frac{1}{25}$	2976	1,470,144
"	"	"	"	"	"	3440	1,699,360
Sept. 5, 4.30	"	"	"	"	"	3190	1,575,860
"	"	"	"	"	"	2602	1,285,388
"	Sept. 15	10	"	"	$\frac{1}{25}$	0	0
"	"	"	"	"	"	0	0
Sept. 6, 4.30	Sept. 9	3	"	0	$\frac{1}{25}$	22,400*	560,000
"	Sept. 16	9	"	0	$\frac{1}{25}$	0	0

per square as near as could be counted.

This seemed to show that the exposure to what was, after all, only diffused light, had very little effect in five and a half hours.

Both flasks were taken in at 5.30, and put on ice for the night at 6 P.M., and remained on ice till 11 A.M. on the 21st, *i.e.*, seventeen hours in dark and on ice. They were then put out again from 11.15 A.M. to 4 P.M., the weather being much brighter, though plenty of white cumulus clouds kept sweeping over the sun.

Before putting out, two plates were made from each flask at 11.30 A.M. on the 21st. Those from F_1 (unexposed) gave 1,149,000 and 646,000; total, 1,795,000; mean, 897,500; numbers very similar to those of the previous day, and indicating that no essential changes had occurred on the ice—possibly a few had succumbed to the rapid cooling.

The two plates from the exposed flask F_2 gave 42,000 and 1,200,000—the last number being too high, as there were numerous invading forms on the plate rendering it difficult to count. Taking the figures as they stand we have, total, 1,242,000; mean, 621,000, which is a reduction on last night's figures.

After exposure on the 21st, two plates were again made from each flask, with the following results.

Of the two plates from the exposed flask F_1, one gave 122,500, and the other 50,750 as the maximum numbers per c.c. Total, 173,250; mean, 86,625 per c.c.

While the unexposed flask gave 1,920,000 and 640. The last low number was obviously due to some blunder; but, even if we take it to reduce the average, we get total, 1,920,640; mean, 960,320 per c.c.

After exposure, the flasks were again put on ice at 9 P.M., and remained there till 12 noon next day, *i.e.*, August 22; they had been at 16° C. in the interval from 4.30 P.M. to 9 P.M.

On the 22nd a plate was taken from each flask at 2.30 to 3 P.M., and the F_1 gave no anthrax colonies at all, though nursed for nearly a week. The other flask gave 990 colonies, which comes to 297,000 per c.c.

On the 23rd, after another seven and a half hours' exposure, plates were again made, one from each, and gave the following numbers—the exposed flask 2 colonies, which = 720 per c.c., and shows that all spores were not yet killed, and the unexposed one 20 colonies, which = 6800 per c.c.

Unfortunately we were compelled to abandon these flasks now; the mere labour of counting within the necessary periods the numerous plates we were making made it imperative that this series should be discontinued. I am now particularly sorry this was so, because it would have been interesting to find if, and when, the light absolutely cleared the water of spores. However, we could not foresee what the tabular *résumé* brings out so clearly. (Table O.)

A point of great importance arises here—not for the first time, but very vividly. That is the gradual, and much slower, but, nevertheless, determined reduction of the spores, even in the dark flask. I am convinced that the principal factor in this is the changes in temperature undergone by the water, which was warmed up to 30° C., or thereabouts, during the day, and cooled to 4° or 5° C., or even lower, during its stay on ice.

§ XIX.

The following experiment (Table P) gives an excellent example of how much can be done by the clear sun of a hot summer day in clearing the water of living spores of anthrax.

A quantity of anthrax spores were carefully rubbed up in about 50 c.c. of sterile-distilled water, on August 16, and the infected water distributed into two Erlenmeyer flasks, marked A*x* and B*x*.

A*x* was exposed to the sun, with a mirror below and behind, from 10 A.M. to 4 P.M., and the sun during the whole period was brilliant. B*x* was placed beside A*x*, but carefully shut in an opaque wooden box.

A sample plate was taken from each flask before exposure, and, after twenty-five and a half hours' incubation at 22—25° C., gave the following numbers. Plate from A*x* = 23,000 colonies = 897,000 per c.c., the plate beginning to liquefy. Plate from B*x*, after the same incubation, was already in an advanced stage of liquefaction, but we satisfied ourselves of at least 5000 visible colonies = 170,000 per c.c. Total of the two, 1,067,000; mean, 533,500, as the minimum number per c.c.

At 4.15 to 4.30 P.M., after six hours' bright insolation of the exposed plate, two sample plates were taken from each flask.

Those from A*x* (exposed) gave 117 and 40 colonies respectively as the maximum numbers we could discover after 72½ hours' incubation, beyond which we could not carry the process. The counting was done twice every day, and every colony actually marked. These numbers give us 4563 and 1360 per c.c. as the maximum; total = 5923; mean = 2961 per c.c.

On the two plates from B*x* (not exposed) we found *at least* 5900 and 6600 respectively, after repeated countings of all the squares. Moreover, these numbers were obtained in forty-eight and a half hours, the liquefaction being so pronounced later that we could not count further. We thus get a *minimum* of 200,600 and 228,400 per c.c.; total, 429,000; mean, 219,500 per c.c.

Even admitting—as of course we do—that these numbers can only be approximations, it is at least clear that the bactericidal power of the sun's rays, even on the spores in sterile water, is far more

Table O.—Insolation of Anthrax

Flask.	Contents.	When filled.	Exposed or not.	Time of exposure.	Number of hours exposed.	Number of plate.	When made.	When examined.
F 1	Anthrax spores in sterile distilled water ½ in. deep	Aug. 20, 11 A.M.	Not	..	0	P 1	Aug. 20, 11 A.M.	Aug. 24, 9 A.M.
F 1	"	"	"	..	0	P 2	"	Aug. 23, 10 A.M.
F 2	"	"	"	..	0	P 3	Aug. 20, 11.30 A.M.	Aug. 23, 11 A.M.
F 2	"	"	"	..	0	P 4	"	"
F 1	"	"	Exposed	Aug. 20, 11.30 A.M. —5 P.M.	5½	P 5	Aug. 20, 5.30 P.M.	Aug. 25, 11.30 A.M.
"	"	"	"	"	"	P 6	"	Aug. 25, 12 noon
F 2	"	"	Not	..	0	P 7	"	"
"	"	"	"	..	0	P 8	"	Aug. 25, 12.30 P.M.
F 1	"	"	Exposed	Aug. 20, 11.30 A.M. —5 P.M.	5½	P 9	Aug. 21, 11.30 A.M.	Aug. 24, 12 noon
"	"	"	"	"	"	P 10	"	Aug. 27, 11 A.M.
F 2	"	"	Not	..	0	P 11	"	Aug. 26, 8 A.M.
"	"	"	"	..	0	P 12	"	Aug. 25, 1 P.M.
F 1	"	"	Exposed	Aug. 20, 11.30 A.M.—5 P.M.; Aug. 21, 11.15 A.M.—4 P.M.	10¼	P 13	Aug. 21, 5.15 P.M.	Aug. 27, 11.30 A.M.
"	"	"	"	"	"	P 14	"	Aug. 27, 12 noon
F 2	"	"	Not	..	0	P 15	Aug. 21, 5.30 P.M.	Aug. 26, 7 A.M.
"	"	"	"	..	0	P 16	"	Aug. 28, 11 A.M.

Spores in Distilled Water.

Hours of incubation.	Temperature of incubation.	Number of colonies on plate.	Number of bacteria per c.c.	Remarks.	Weather.
94	18—20°	5000	1,950,000	1 c.c. of water from the flask diluted to 10 c.c. with sterile distilled water, and the plate made from a drop of the mixture.	
71	"	6000	2,340,000	"	
71½	"	6000	2,160,000	"	
"	"	5700	2,052,000	"	
114	"	5000	1,700,000	"	Cloud all the time, and a good deal of rain.
114½	"	1000	340,000	Badly levelled plate; anthrax colonies all up at one side.	"
"	"	1700	595,000		
115	,	3700	1,295,000		
72½	"	3350	1,149,000	Two liquefying, twenty-five *large* anthrax, and possibly very many smaller, very numerous small colonies round the edge. Diluted	Flask had been placed on ice overnight, 6 P.M. to 11 A.M.
143½	"	1900	646,000	Diluted. Nine moulds and four other foreign forms.	"
116½	"	140	42,000	Diluted.	"
97½	"	4000	1,200,000	Diluted	Numerous intruders.
138½	"	353	122,500	Diluted. One mould, forty-two (?) anthrax	Clear sunshine, with clouds occasionally. About three hours sun.
138¾	"	145	50,750	Diluted.	
109½	"	6000	Total 1,920,000	Diluted.	
161½	"	2	640	Diluted	This plate is so abnormal that there was obviously some blunder.

Table O—

Flask.	Contents.	When filled.	Exposed or not.	Time of exposure.	Number of hours exposed.	Number of plate.	When made.	When examined.
F 1	Anthrax spores in distilled water ½ in. deep.	Aug. 20, 11 A.M.	Exposed	Aug. 20, 11.30 A.M.—5 P.M.; Aug. 21, 11.15 A.M.—4 P.M.	10½	P 21	Aug. 22, 2.30 P.M.	Aug. 29, 12 noon
F 2	"	"	Not	..	0	P 22	Aug. 22, 3 P.M.	Aug. 27, 12.30 P.M.
F 2	"	"	"	..	0	P 36	"	"
F 1	"	"	Exposed	Aug. 20, 11.30 A.M.—5 P.M.; Aug. 21, 11.15 A.M.—4 P.M.; Aug. 23, 9 A.M.—4.30 P.M.	17½	P 35	Aug. 23, 6.30 P.M.	Aug. 29, 12 noon

energetic than has been commonly supposed, and the results also suggest that some spores die off very quickly, even in the dark, when put into sterile water—a fact long known.

On August 18, we started a similar experiment to the last, using Thames water, freshly collected, instead of sterilised distilled water; but this had to be abandoned owing to the difficulties with the rapidly-developing liquefying forms at the temperatures necessary for growing the anthrax. There was nothing in the results to contradict previous experience, but the details are of little value.

§ XX.

On October 6 I exposed a tube of broth, infected with a loopful of colonies β—from gelatine stab-culture—from 9 A.M. to 4 P.M. (seven hours), over a mirror, to the sun; the sky was clear, and sun bright till about 1 P.M., and then duller. An exactly similar tube was wrapped in foil and black paper, and placed by the side of the above.

At 4 P.M. a plate was made from each tube, and incubated at 15° C.; a stab-culture from each was also made and kept at 15° C., and the original tubes were placed at 20—22° C.

Taking the plates first. In forty hours the plate from unexposed tube showed numerous colonies, from 0·5 mm. to 1 mm. diameter,

continued.

Hours of incubation.	Temperature of incubation.	Number of colonies on plate.	Number of bacteria per c.c.	Remarks.	Weather.
165½	18—20°	0 Anthrax	0 Anthrax	Diluted. Six small strangers	Flask on ice, Aug. 21, 9 P.M., to Aug. 22, 12 noon.
117½	"	990	297,000	Diluted	"
"	"	2	720	"	"
137½	"	20	6,800	"	One hour sun 3—4 P.M. Flask on ice 3 P.M., Aug. 22, to 9 A.M., Aug. 23.

and quite visible to the unaided eye, the larger ones opening out and swarming vigorously as liquefaction began. A greenish shimmer showed where the colonies were very dense. In sixty-four hours the colonies were running together, and liquefying rapidly, and in eighty-six hours the whole gelatine was liquid and watery.

The plate from the insolated tube, treated in exactly the same way, showed no trace of colonies in forty hours, and only one or two minute colonies, invisible to the unaided eye, could be detected under the microscope in sixty-four hours. Even after seven days the morula-like, dense, granular colonies only showed traces of opening out and the first beginnings of liquefaction.

Of the stab-cultures, that from the covered tube showed the beginnings of growth in forty hours, and had formed a thistle-head funnel of liquefaction in sixty-four hours, which rapidly extended in eighty-eight hours.

That from the insolated tube, on the contrary, showed no trace of growth in forty hours, or even in sixty-four hours to eighty-eight hours.

Of course it may be objected that the difference here was entirely due to the stab-infection having carried in so few living germs in the latter case; but to this it must be replied, that the plates show

Table P.

Flask.	Contents.	When filled.	Exposed or not.	Time of exposure.	Hours exposed.	Number of plate.	When made.	When examined.	Hours of incubation.	Temperature of incubation.	Number of colonies.	Number of bacteria per c.c.	Remarks.
A*x*	Anthrax spores in sterile distilled water	Aug. 16, 10 A.M.	Not	..	0	× A I	Aug. 16, 10 A.M.	Aug. 18, 11.20 A.M.	25¼	22·25	23,000	897,000	Liquefied.
"	"	"	Exposed with mirror	Aug. 16, 10 A.M.—4 P.M.	6	× A II	Aug. 16, 4.30 P.M.	Aug. 19, 4 P.M.	72¼	..	117	4,563	
"	"	"	"	"	6	× A II′	"	"	"	..	40	1,360	
B*x*	Anthrax spores in sterile distilled water	Aug. 16, 10 A.M.	Not	..	0	× B I	Aug. 16, 10 A.M.	Aug. 18, 11 A.M.	25	..	5,000	170,000	Liquefied and certainly underestimated.
"	"	"	"	..	0	× B II	Aug. 16, 4.30 P.M.	Aug. 18, 5 P.M.	24¼	..	5,900	200,600	
"	"	"	"	Aug. 16, 10 A.M. to 4 P.M.	0	× B II′	"	Aug. 18, 4 P.M.	23¼	..	6,600	228,400	

clearly that the *individual colonies* are weakened by the light-action, and the results with the original tubes (given below) may also be consulted.

Turning now to the original broth cultures, placed at 20—22° C. In eighteen hours the darkened tube was distinctly turbid, whereas that exposed to the sun was perfectly clear; and the same was the case at the end of forty hours. In sixty-four hours both tubes were turbid, but the exposed one far less so than the other. Later on it was impossible to distinguish between them.

It seems to me impossible to avoid the conclusion that the light-action so weakens the metabolism of the insolated cells that they grow and divide more slowly, and dissolve the gelatine more feebly, and possibly this weakening effect is transmitted to the cells to which they give rise by division. As time passes, however, the cells gradually recover their vigour in the dark, and where plenty of food material is accessible.

That this latter statement is true the following experiment proves:—On October 18 I took the two broth-tubes of Colony β, referred to above, both of which had been in the dark, side by side, since October 6. Broth-tubes were exactly similar at the beginning, as we have seen, but one of them had been exposed, on October 6, for seven hours to the sunlight.

On making stab-cultures from these tubes, no difference could be detected between their behaviour, both began to liquefy the gelatine in forty-eight hours in the typical, thistle-head funnel form.

This seems to show clearly, also, that the broth has not been injured as a medium for culture by its exposure to light.

In order to meet the objection that the above results were due to the using of broth-cultures, I repeated them as follows on October 10:—

Two tubes of sterile water, infected from the turbid broth-culture kept in the dark since October 6, were suspended, as already described—one exposed over a mirror to the bright direct sunlight from 10 A.M. to 2 P.M., the other wrapped up, and placed beside it. At 2 P.M. plates and stab-cultures were made from each tube.

The results were the same as before. The plates from the unexposed tubes showed numerous liquefying colonies in forty-eight hours, and were completely liquefied on the third day; the plates from the insolated tube showed only very few colonies, and these not till the third day, and they were denser, more granular, and without any signs of liquefaction for several days. To take a concrete case: a plate made with 1 drop (1/30 c.c.) from the dark tube showed about 5000 colonies in forty-eight hours, and these were already beginning to run; the whole of the gelatine was liquefied in another twenty-four hours; the corresponding plate made with 1 drop (1/24 c.c.)

from the insolated tube showed three colonies only on the third day, and these were very small, more densely granular, and irregular in outline than the normal colonies, and showed no traces of liquefaction even on the fourth day. On the fifth day the liquefaction was beginning, but even after fourteen days one of the three colonies was only just breaking up.

The stab-cultures gave similar results. Those from the darkened tube showed a distinct thistle-head funnel of liquefaction in three days, whereas the feeblest signs that infection had really occurred were all I could get in the same time from the insolated cultures.

Here, again, however, the two sets of tubes gradually become alike, evidently because the at first enfeebled cells gradually regain their vigour, and once more rapidly peptonise the medium.

The experiments with water were repeated on October 12, all the arrangements being as before.

The exposed tube was out from 9 A.M. to 3 P.M. in brilliant sunshine the whole time practically.

After three hours' exposure, a plate was made. That from the dark tube showed colonies, visible only under the microscope, in nineteen hours, and in two days the whole of the gelatine was liquid like water.

The plate from the lighted tube showed no signs until the third day; on the fourth day six colonies had made their appearance, but these only began to soften the gelatine around some days later, and even on the twelfth day two of the six colonies were still circular depressions, though the other four had liquefied the gelatine some distance round.

After six hours' exposure, further plates were made from the above tubes on October 12.

As before, colonies were visible with the lens on the plate made from the dark tube in sixteen hours, and before the end of the second day the whole of the gelatine was liquefied like water.

On the plate from the light tube five slowly-developing colonies had appeared by the fourth day, one of which showed feeble signs of liquefaction next day. But even after twelve days only three of these colonies were vigorously liquefying the gelatine, the other two being still compact and circular, though one of them lay in a slight depression.

Stab-cultures were also made at the end of the six hours' exposure on October 12, with the results as before. In the case of the culture from the dark tube, the funnel of liquefaction had reached the walls of the tube, and liquefied one-eighth of an inch of gelatine in four days, whereas that from the lighted tube, in the same period and side by side, had not even begun to liquefy the gelatine, though the infection had taken.

I also, at the end of the experiment on October 12, cautiously emptied each of the water tubes, and drained it until only a drop remained, and then filled up with sterile broth. After sixteen hours the dark tube was distinctly turbid, whereas no trace of turbidity appeared in the insolated one till after forty-eight hours. Both were equally turbid on the fourth day.

Of course I recognise that this last result, and that obtained with the stab-cultures, is attributable to the *numbers* of still living germs added to the gelatine and broth respectively, and the experiments only go to prove once more, but in a very decisive manner, what mortality the sun had occasioned in the exposed tube—for we must remember, each tube contained practically the same enormous numbers at the start.

§ XXI.

On October 6 a loopful of a markedly liquefying form, marked in my notes as colony θ, was placed in each of two tubes of broth, and placed in the sun from 9 A.M. to 4 P.M. One tube was exposed over a mirror, the other side by side, but wrapped in foil and black paper. The sun shone brightly from 9 to 1, and then was obscured by clouds.

At 4 P.M. a plate and a stab-culture from each tube were made, and the tubes put at 20° to 22° C. in the dark incubator. The plate and stab-cultures were put at 15° C. in the dark.

Taking the broth tubes first. Both were already turbid in eighteen hours, more densely so after forty hours, so that no difference between them could be detected.

Of the stab-cultures, that from the dark tube had taken in forty hours, while the one from the lighted tube showed no signs.

In sixty-four hours the non-illuminated culture had developed a thistle-head liquefaction funnel, whereas no trace was visible in the other. In eighty-eight hours the liquefaction had proceeded rapidly in the former tube; only one feeble colony was visible in the tube from the insolated broth. In the course of a week or so no further difference could be made out.

With regard to the plates. That from the insolated tube showed no trace until sixty-four hours had passed, when two or three colonies $\frac{1}{4}$ to $\frac{1}{2}$ mm. diameter were seen. In eighty-six hours these were somewhat like young anthrax colonies, each with a slight depression. On the sixth day liquefaction of the gelatine was slowly evincing itself.

The plate from the dark tube showed evident colonies, $\frac{3}{8}$ to $\frac{1}{2}$, and even 1 mm. in diameter, in forty hours; and in sixty-four hours they averaged 2 to 5 mm. in diameter, and were liquefying rapidly. These circular colonies were less rapid than those of colony β at the

same time, however, and whiter in colour. In eighty-eight hours the whole of the gelatine was completely liquefied.

§ XXII.

On October 30 about 200 c.c. of sterilised Thames water were strongly infected with the bacillus called *B. arborescens* by Frankland, taken from a vigorous culture.

The infected liquid was divided equally into two Erlenmeyer flasks, one of which was at once wrapped in tin-foil and black paper. The other was supported above a plane mirror, and placed so as to obtain the maximum amount of direct sunshine available from 10 A.M. to 5 P.M. that day; from 10 to 1 the sunshine was hot and bright, and a control flask showed that the water rose to nearly 20° C., but the afternoon was dull and cloudy, and the temperature fell to 12° C. The temperature in the covered flask, placed side by side with the exposed one, rose and fell so nearly exactly with that of the latter, that no stress can be laid on the difference.

Before commencing the experiment at 10 A.M., sample plates were made of the infected water; and at 5 P.M. two plates were made from each flask—exposed and unexposed.

The plates from the freshly infected material showed the usual rapidly growing, loose, thread-like colonies in forty-eight hours, and in three days the gelatine was entirely liquefied to a watery fluid.

The plates from the flask, wrapped in foil and paper, and sheltered from the direct rays of the sun, behaved similarly, the colonies being a trifle more compact in shape, but equally rapid in liquefying the gelatine completely.

But the plates from the exposed flask differed from the first onwards from those not insolated. Thus no colonies were visible in forty-eight hours, a period during which the plates from the unexposed flasks exhibited numerous typical colonies; the colonies appeared here twenty-four hours later, clearly showing the effects of inhibition due to the light.

Secondly, when the colonies did make their appearance they were more compact, and instead of shooting out in all directions and covering the plate with a meshwork of fine branches, rapidly liquefying the gelatine, as in the case of the unexposed specimens, the mode of growth was so affected that on the fourth day they had developed into beautifully circular yellowish colonies, zoned, and radially striated, and only just softening the gelatine. It was not, indeed, till the sixth day that liquefaction set in generally.

I explain the differences as follows. The light retards the growth of the living bacilli, owing to some action on their protoplasm which induces interference with the metabolic processes on which growth

depends; this causes the cell-chains to be so modified in length, direction, and rapidity of development, that the colony formed from the insolated germ is weaker and more condensed, or compact, than normally. Thus result the differences in the naked eye characters of the colonies, which may go so far that the total aspect on the gelatine plate is altered.

Some clue to the action may perhaps be got eventually by following up the fact that one consequence of the light action is to weaken the enzyme action of the bacterium—for the enfeebled liquefying power is an expression of enfeebled enzyme power—either by so altering the protoplasmic machinery that less enzyme is secreted, or by so acting on the enzyme that its power of converting the medium is altered. The lessened enzyme power of course implies less power to obtain its food from the medium, and so the progeny developed from the germ started with are also feebler than the normal one.

In the cases quoted, however, the colony gradually becomes more normal as regards its enzyme power, especially in the dark, because the successively developed new cells become stronger and stronger as they are fed by the nutrient gelatine, and at last the differences are equalised.

The only source of error in the above conclusion that I could think of, was the possibility that the rapid running of the colonies into thin filaments in the first case is facilitated by the quicker liquefaction of the gelatine, and that this liquefaction is, in turn, more rapid, because there are so many more colonies per drop in the unexposed flasks, because the majority of the bacilli in the exposed flasks are killed.

I accordingly made plates in which I diluted the samples from the unexposed flasks five, ten, and even twenty times as much as the samples from the exposed flasks, and so brought the numbers of colonies on each plate approximately equal. Of course it may be replied here that the differences of dilution may bring about differences in development; but experience shows that increased dilution tends to *inhibition* of colonies, and so I think the fact that I still get the differences in the colonies already described, strengthens rather than weakens the evidence that the alteration in the character of the colonies from exposed flasks is really due to the action of the light.

On November 7, which turned out a beautifully fine day, with clear blue sky and bright sun, tubes of sterile distilled water were infected with cultures of a yellow bacillus marked *, a large white one marked ◇, and a violet bacillus, common in the Thames. In each case, two similar tubes were prepared, exactly alike, one of which was exposed over a mirror, from 10.30 A.M. to 3.30 P.M.; while the other was wrapped in foil and black paper, and placed by the side of the exposed one. The temperature recorded in control tubes was 10—12° C.

At 4 P.M., after five hours' exposure, plates were made, and incubated at 15° C.

Taking the violet bacillus first. Nothing appeared on either of the two plates made from the exposed tubes, although they were kept till November 24, *i.e.*, seventeen days. On the plate made from the dark tube, two white colonies were visible in forty-eight hours, and on the 13th—*i.e.*, after six days—three colonies were seen. These remained white until December 1, when one of them began to show the violet line. On December 6 this was more pronounced.

The experiment, therefore, showed negative results only, and I regard it as probable that the mere immersion in water injures the bacillus. On the other hand, it is possible I did not use sufficient material in making the plates.

Thus the plates of exposed tube = 1/28th c.c. of 7/28 dilution, and that of dark one = 1/27th c.c. of 1/27 dilution, thus giving only 2187 per c.c. even in the unexposed tube, making it probable that the last suggestion is the right one.

Now, as regards the yellow bacillus, Colony *. Of the two plates made from the insolated tube, one was contaminated, and yielded no results, except that plenty of colonies appeared; the other failed utterly. The plate from the dark tube showed innumerable typical colonies, and was completely liquefied on the fourth day.

The experiments consequently must be regarded as negative.

The plates of Colony ⟐ behaved as follows. That from the dark tube gave a typical series of colonies—about 500 = $500 \times 28 \times 28$ = 392,000 per c.c., softening the gelatine in two days.

That from the lighted tube gave far fewer—about $100 \times 25 \times 5$ = 12,500 per c.c.—colonies, and these smaller, showing evident retardation; otherwise no results.

On November 12 the above experiment was repeated with Colony *, Colony ♓, and the rosy-red bacillus δ, exactly as before. The day was cold and bright, though some haze appeared after 1 P.M. Exposure 10.30 to 3.30 as before, and thermometers = 10—12° C.

The plates of Colony * behaved as follows. That from the dark tube was liquefied by numerous colonies, which appeared on the second day. On the fourth day all the gelatine was liquefied.

The plates (two) made from the insolated tube only showed slight retardation, and liquefied also on the fourth day, though more slowly.

No difference between illuminated and dark tubes could be made out in the case of Colony ♓, except slight retardation on the plates from the former.

The results with Colony δ were still more indecisive, and I could not draw any conclusions as to distinct light action.

It appears probable that considerable differences will be found between the various forms in this respect.

§ XXIII.

In the following tables I give the determinations of the numbers of all bacteria found in three series of analyses of samples of Thames water in August, October, and December, 1893. As will be seen, they confirm the results of other observers, in so far that they show that the number per c.c. is considerably greater in winter than in summer.

There is nothing special to note as regards the methods employed, which were those ordinarily in use, beyond the fact that we counted *every* colony on each plate, instead of estimating the averages from a few squares.

The greatest care was taken to have the gelatine made up exactly alike in every case; further information as to details is supplied by the tables.

Table Q.—Number of Bacteria in 1 c.c. Thames Water in August, 1893.

Date.	Plate.	Age of water.	Temperature of incubation.	Time incubated.	Dilute or not.	No. of drops.	Colonies on plate.	Calculated No. of bacteria per 1 c.c.	Averages.
		° C.	° C.	days.		c.c.			
12.8.93	A 1	1 h. at 15—18	12—15	2	not	$\frac{1}{39}$	40	1560	1446
”	B 1	”	”	”	”	$\frac{1}{34}$	50	1700	
”	C 1	”	”	”	”	$\frac{1}{36}$	30	1080	
”	A 1	”	”	3	”	$\frac{1}{39}$	95	3705	2680
”	C 1	”	”	”	”	$\frac{1}{36}$	46	1656	
14.8.93	A 1′	”	15—18	2	”	$\frac{1}{39}$	45	1755	1434
”	A 1‴	”	”	”	”	$\frac{1}{39}$	36	1404	
”	B 1′	”	”	”	”	$\frac{1}{34}$	20	680	
”	B 1‴	”	”	”	”	$\frac{1}{34}$	64	2176	
”	C 1″	”	”	”	”	$\frac{1}{34}$	34	1156	
22.8.93	P. 17	”	16—18	2	”	$\frac{1}{32}$	25	800	1104
”	P. 18	”	”	”	”	$\frac{1}{32}$	44	1408	
”	P. 19	”	”	”	”	$\frac{1}{34}$	22	748	
”	P. 20	”	”	”	”	$\frac{1}{34}$	43	1462	
”	P. 17	”	”	3	”	$\frac{1}{32}$	26	832	1189
”	P. 18	”	”	”	”	$\frac{1}{32}$	60	1920	
”	P. 19	”	”	”	”	$\frac{1}{34}$	24	816	
24.8.93	P. 37	”	16—18	4	$\frac{1}{10}$	$\frac{1}{33}$	6	1980	1980
”	P. 38	”	”	”	”	”	5	1650	
”	P. 39	”	”	”	”	”	4	1320	
”	P. 40	”	”	”	”	”	9	2970	
25.8.93	P. 45	”	”	3	not	$\frac{1}{34}$	49	1666	1530
”	P. 46	”	”	”	”	”	38	1292	
”	P. 47	”	”	”	”	”	41	1394	
”	P. 48	”	”	”	”	”	52	1768	

25.8.93	P. 45	„	„	5	„	$\frac{1}{34}$	100	3400	3356
„	P. 46	„	„	„	„	„	64	2716	
„	P. 47	„	„	„	„	„	79	2686	
„	P. 48	„	„	„	„	„	136	4624	
28.8.93	P. 55	„	„	3	$\frac{1}{10}$	$\frac{1}{33}$	5	1650	1555
„	P. 56	„	„	„	„	„	4	1320	
„	P. 57	„	„	„	„	„	4	1320	
„	P. 58	„	„	„	„	„	6	1980	
„	P. 59	„	„	„	„	„	6	1980	
„	P. 61	„	„	„	„	„	4	1320	
„	P. 62	„	„	„	„	„	4	1320	
28.8.93	P. 56	„	„	4	$\frac{1}{10}$	$\frac{1}{33}$	11	3630	3300
„	P. 57	„	„	„	„	„	7	2310	
„	P. 58	„	„	„	„	„	10	3300	
„	P. 59	„	„	„	„	„	12	3960	
28.8.93	P. 56	„	„	5	$\frac{1}{10}$	$\frac{1}{33}$	14	4620	3795
„	P. 57	„	„	„	„	„	7	2310	
„	P. 58	„	„	„	„	„	13	4290	
„	P. 59	„	„	„	„	„	12	3960	
29.8.93	P. 64	„	„	2	not	$\frac{1}{34}$	6	204	136
„	P. 67	„	„	„	„	„	2	68	
30.8.93	78	1 h. at 16—18	16—18	3	$\frac{1}{10}$	$\frac{1}{30}$	5	1500	1350
„	79	„	„	„	„	„	1	300	
„	80	„	„	„	„	„	3	900	
„	81	„	„	„	„	„	4	1200	
„	82	„	„	„	„	„	7	2100	
„	83	„	„	„	„	„	3	900	
„	84	„	„	„	„	„	10	3000	
„	85	„	„	„	„	„	3	900	
30.8.93	79	1 h. at 16—18	16—18	4	$\frac{1}{10}$	$\frac{1}{30}$	1	300	1713
„	80	„	„	„	„	„	3	900	
„	81	„	„	„	„	„	9	2700	
„	82	„	„	„	„	„	7	2100	
„	83	„	„	„	„	„	5	1500	
„	84	„	„	„	„	„	10	3000	
„	85	„	„	„	„	„	5	1500	

Table Q—*continued.*

Date.	Plate.	Age of water.	Temperature of incubation.	Time incubated.	Dilute or not.	No. of drops.	Colonies on plate.	Calculated No. of bacteria per 1 c.c.	Averages.
		° C.	° C.	days.		c.c.			
30.8.93	78	1 h. at 16—18	16—18	6	$\frac{1}{10}$	$\frac{1}{30}$	6	1800	3037
"	79	"	"	"	"	"	2	600	
"	80	"	"	"	"	"	4	1200	
"	81	"	"	"	"	"	15	4500	
"	82	"	"	"	"	"	14	4200	
"	83	"	"	"	"	"	10	3000	
"	84	"	"	"	"	"	21	6300	
"	85	"	"	"	"	"	9	2700	

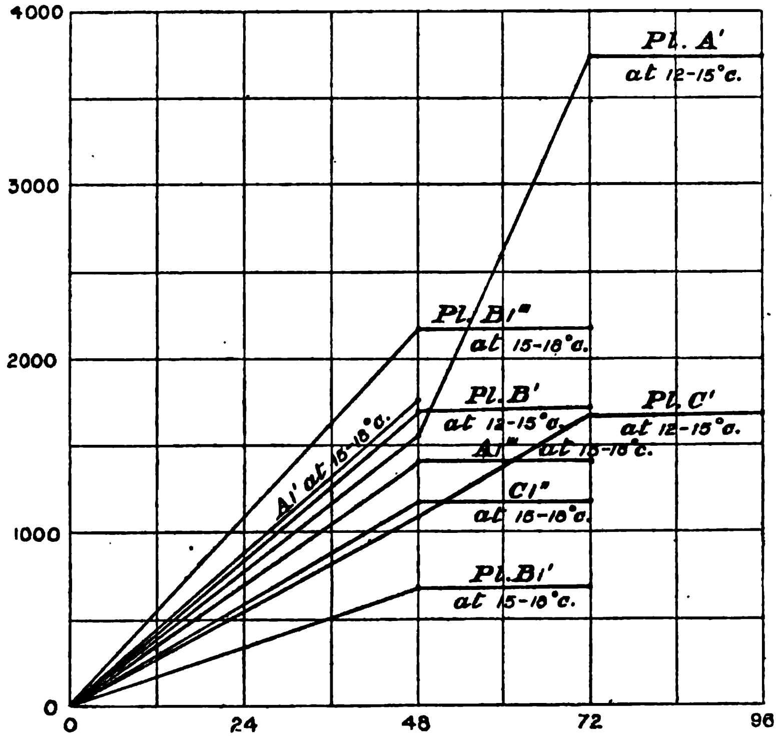


DIAGRAM I.—Curves of Table Q, showing total bacteria at 12—15° C. and 15—18° C. in August. Abscissæ = hours of incubation of plates; ordinates = numbers of colonies per 1 c.c.

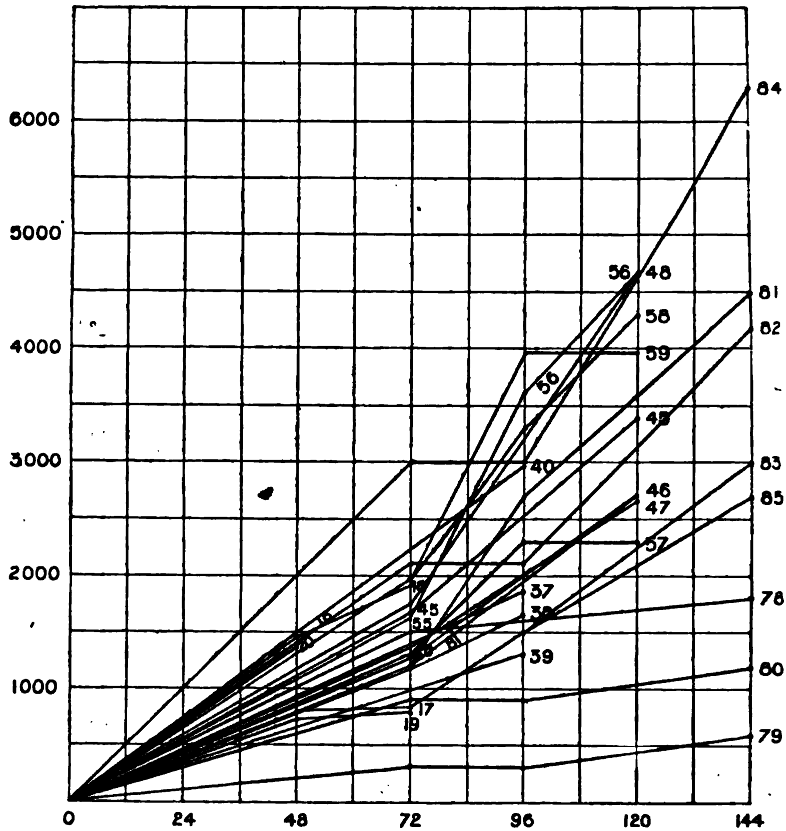


Diagram II.—Curves of Table Q, showing total bacteria at 16—18° C. in August. Abscissæ = hours of incubation of plates; ordinates = numbers of colonies of bacteria per 1 c.c.

Quantitative Analysis of Thames Water in August, 1893 (*Table Q*).

If we analyse these August numbers, we get as the average of 14 two-days' plates, 1157; of 24 three-days' plates, we have 1530; of 15 four-days' plates, we have 2208; of 8 five-days' plates, we get 3575; and of 8 six-days' plates, we have 3037, as the approximate averages. Of course these numbers can only be regarded as approximations, but I think they are of use as indicating what might be looked for if the necessary large series of observations could be made over several years; and they certainly serve to put us on our guard against placing too much confidence in the gelatine method unless a great number of observations are made, extending over a long period. Of course the great difficulty to be contended against is that of keeping the plates long enough; if only *one* badly liquefying form is present, it ruins the plates before the slowly developing ones germinate out.

Quantitative Analysis of Thames Water in October, 1893 (*Table R*).

The following table gives the details of the analysis for October, and again I have recorded all the points. The chief feature ot interest is the much longer time during which the plates could be cultivated, in spite of the prevalence of liquefying forms, where care was taken to dilute sufficiently.

Table R.—Number of Bacteria in 1 c.c. Thames Water in October, 1893.

Date.	Plate.	Age of water.	Temperature of incubation.	Time incubated.	Diluted or not.	No. of drops.	Colonies on plate.	Calculated No. of bacteria per 1 c.c.	Averages.	Total average.
		° C.	° C.	hours.		c.c.				
21 10.93	E	$\frac{1}{4}$ hour at 15	16—17	24	$\frac{1}{5}$	$\frac{1}{27}$	1	135	169	317
”	F	”	”	”	”	”	1	135		
”	G	”	”	”	”	”	2	270		
”	H	”	”	”	”	”	1	135		
”	L	”	”	”	$\frac{1}{3}$	$\frac{1}{27}$	3	243	466	
”	M	”	”	”	”	”	10	810		
”	N	”	”	”	”	”	3	243		
”	P	”	”	”	”	$\frac{2}{27}$	14	567		
”	E	”	”	48	$\frac{1}{5}$	$\frac{1}{27}$	11	1,485	1,494	1,876
”	F	”	”	”	”	”	9	1,115		
”	G	”	”	”	”	”	14	1,890		
”	H	”	”	”	”	”	11	1,485		
”	L	”	”	”	$\frac{1}{3}$	$\frac{1}{27}$	18	1,458	2,258	
”	M	”	”	”	”	”	32	2,592		
”	N	”	”	”	”	”	26	2,106		
”	P	”	”	”	”	$\frac{2}{27}$	71	2,875		
”	E	”	”	72	$\frac{1}{5}$	$\frac{1}{27}$	17	2,295	2,639	3,309
”	F	”	”	”	”	”	23	3,405		
”	G	”	”	”	”	”	21	2,830		
”	H	”	”	”	”	”	15	2,025		
”	L	”	”	”	$\frac{1}{3}$	$\frac{1}{27}$	43	3,483	3,979	
”	M	”	”	”	”	”	49	3,969		
”	N	”	”	”	”	”	51	4,131		
”	P	”	”	”	”	$\frac{2}{27}$	107	4,333		
”	E	”	”	96	$\frac{1}{5}$	$\frac{1}{27}$	27	3,645	4,151	4,920
”	F	”	”	”	”	”	30	4,050		
”	G	”	”	”	”	”	40	5,400		
”	H	”	”	”	”	”	26	3,510		
”	L	”	”	”	$\frac{1}{3}$	$\frac{1}{27}$	64	5,184	5,690	
”	M	”	”	”	”	”	75	6,075		
”	N	”	”	”	”	”	65	5,265		
”	P	”	”	”	”	$\frac{2}{27}$	154	6,237		

,,	E	,,	,,	120	$\frac{1}{5}$	$\frac{2}{37}$	34	4,590	5,265	6,692
,,	F	,,	,,	,,	,,	,,	44	5,940		
,,	G	,,	,,	,,	,,	,,	49	6,615		
,,	H	,,	,,	,,	,,	,,	29	3,915		
,,	L	,,	,,	,,	$\frac{1}{5}$	$\frac{1}{37}$	105	8,505	8,120	
,,	M	,,	,,	,,	,,	,,	104	8,424		
,,	N	,,	,,	,,	,,	,,	86	6,966		
,,	P	,,	,,	,,	,,	$\frac{2}{37}$	212	8,586		
,,	E	,,	,,	144	$\frac{1}{5}$	$\frac{1}{37}$	34	4,590	5,265	6,905
,,	F	,,	,,	,,	,,	,,	44	5,940		
,,	G	,,	,,	,,	,,	,,	49	6,615		
,,	H	,,	,,	,,	,,	,,	29	3,915		
,,	L	,,	,,	,,	$\frac{1}{5}$	$\frac{1}{37}$	108	8,748	8,545	
,,	M	,,	,,	,,	,,	,,	104	8,424		
,,	N	,,	,,	,,	,,	,,	101	8,181		
,,	P	,,	,,	,,	,,	$\frac{2}{37}$	218	8,829		
,,	E	,,	,,	168	$\frac{1}{5}$	$\frac{1}{37}$	35	4,725	5,670	7,209
,,	F	,,	,,	,,	,,	,,	48	6,480		
,,	G	,,	,,	,,	,,	,,	56	7,560		
,,	H	,,	,,	,,	,,	,,	29	3,915		
,,	L	,,	,,	,,	$\frac{1}{5}$	$\frac{1}{37}$	108	8,748	8,748	
,,	M	,,	,,	,,	,,	,,	111	8,991		
,,	N	,,	,,	,,	,,	,,	105	8,505		
,,	A	,,	20—22	24	$\frac{1}{5}$	$\frac{1}{37}$	3	405	742	1,120
,,	B	,,	,,	,,	,,	,,	5	675		
,,	C	,,	,,	,,	,,	,,	4	540		
,,	D	,,	,,	,,	,,	,,	10	1,350		
,,	I	,,	,,	,,	$\frac{1}{5}$	$\frac{1}{37}$	20	1,620	1,498	
,,	J	,,	,,	,,	,,	,,	15	1,215		
,,	K	,,	,,	,,	,,	,,	18	1,458		
,,	O	,,	,,	,,	,,	$\frac{2}{37}$	42	1,701		
,,	A	,,	,,	48	$\frac{1}{5}$	$\frac{1}{37}$	12	1,620	2,464	2,884
,,	B	,,	,,	,,	,,	,,	13	1,755		
,,	C	,,	,,	,,	,,	,,	25	3,375		
,,	D	,,	,,	,,	,,	,,	23	3,108		
,,	I	,,	,,	,,	$\frac{1}{5}$	,,	40	3,240	3,304	
,,	J	,,	,,	,,	,,	,,	41	3,321		
,,	K	,,	,,	,,	,,	,,	52	3,212		
,,	O	,,	,,	,,	,,	$\frac{2}{37}$	85	3,442		

Table R—*continued.*

Date.	Plate.	Age of water.	Temperature of incubation.	Time incubated.	Diluted or not.	No. of drops.	Colonies on plate.	Calculated No. of bacteria per 1 c.c.	Averages.	Total average.
		° C.	° C.	hours.		c.c.				
20.10.93	A	$\frac{1}{4}$ hour at 15	20—22	72	$\frac{1}{5}$	$\frac{1}{27}$	15	2,025	3,571	4,696
"	B	"	"	"	"	"	20	2,700		
"	C	"	"	"	"	"	41	5,508		
"	D	"	"	"	"	"	30	4,050		
"	I	"	"	"	$\frac{1}{5}$	$\frac{1}{27}$	80	6,480	5,822	
"	K	"	"	"	"	"	76	6,156		
"	J	"	"	"	"	"	74	5,994		
"	O	"	"	"	"	$\frac{2}{27}$	116	4,657		
"	A	"	"	96	$\frac{1}{5}$	$\frac{1}{27}$	29	3,915	5,737	7,546
"	B	"	"	"	"	"	52	7,020		
"	C	"	"	"	"	"	50	6,750		
"	D	"	"	"	"	"	39	5,265		
"	I	"	"	"	$\frac{1}{5}$	$\frac{1}{27}$	128	10,368	9,355	
"	K	"	"	"	"	"	103	8,343		
"	A	"	"	120	$\frac{1}{5}$	$\frac{1}{27}$	34	4,590	6,885	9,963
"	B	"	"	"	"	"	60	8,100		
"	C	"	"	"	"	"	55	7,425		
"	D	"	"	"	"	"	55	7,425		
"	I	"	"	"	$\frac{1}{5}$	$\frac{1}{27}$	157	12,717	13,041	
"	K	"	"	"	"	"	165	13,365		
"	A	"	"	144	$\frac{1}{5}$	$\frac{1}{27}$	45	6,075	7,830	12,015
"	B	"	"	"	"	"	71	9,585		
"	I	"	"	"	$\frac{1}{5}$	$\frac{1}{27}$	190	15,390	16,200	
"	K	"	"	"	"	"	210	17,010		
"	A	"	"	168	$\frac{1}{5}$	$\frac{1}{27}$	56	7,560	9,450	13,027
"	B	"	"	"	"	"	84	11,340		
"	I	"	"	"	$\frac{1}{5}$	$\frac{1}{27}$	200	16,200	10,605	
"	K	"	"	"	"	"	210	17,010		

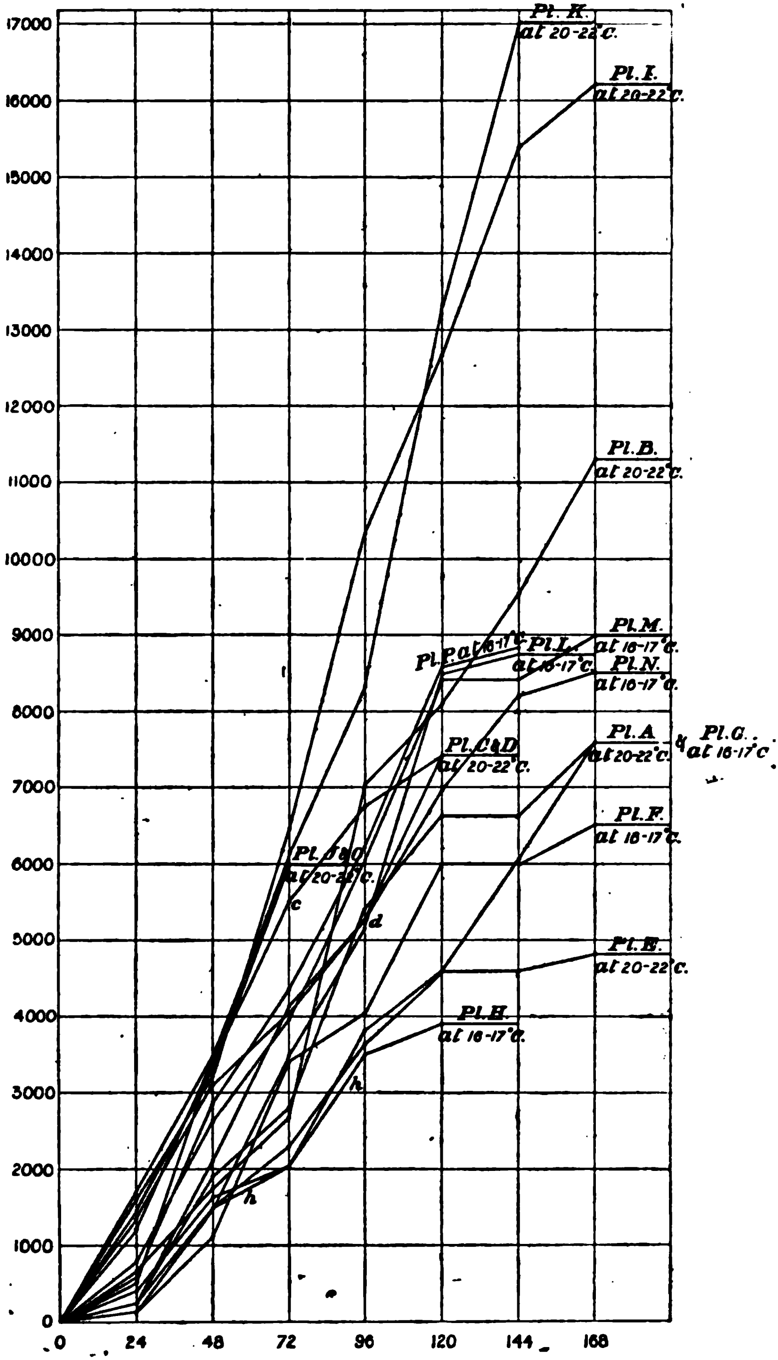


DIAGRAM III.—Curves of Table R, showing total bacteria at 16—17° and 20—22° C. in October. Abscissæ = hours of incubation; ordinates = numbers of colonies per 1 c.c.

Quantitative Analysis of Thames Water in December.

On December 27, 1893, a cloudy and misty cold morning following on a series of bright sunny cold days, a sample of Thames water was taken at 11 A.M., and immediately conveyed to the laboratory, and plates made as follows. The water on collection was at nearly 8° C.

Twenty-two plates in all were made, of which twelve were incubated at 8—12° C., the temperature of the laboratory, and ten at 18—20° C. in the incubator. There were a good many changes of temperature from day to day in the case of the former, though but slight and slow alterations in the latter case.

The following tables give the results of the plate cultures, every colony visible with the hand lens being counted each day as before; and this time the low temperature cultures were continued for 240 hours (ten days), in order to see how far the numbers would eventually approximate to those of the higher temperature plates.

An innovation was made in the case of the plates marked R_1R_2, S_1S_2, T_1T_2. Here I used 1 drop of the Thames water between each pair of plates. The drop was allowed to fall as usual into the tube of gelatine, then a second tube of sterile gelatine emptied into the infected tube; then the contents were distributed rapidly into two plates, the countings of which must be taken together as is done in the table. It is noteworthy how much higher the final numbers are in these plates as compared with others, no doubt chiefly owing to the more complete separation of the bacteria from which the colonies arise, and to their having more space to develope in.

Table S.—Number of Bacteria in 1 c.c. Thames Water in December, 1893.

Date.	Plate.	Age of water.	Temperature of incubation.	Time incubated.	Diluted or not.	Number of drops.	Colonies on plate.	Calculated number of bacteria per 1 c.c.	Averages.	Total average.	
				hours.		c.c.					
Dec. 28, 1893	*a*	½ hour at 8 to 10°	8—12° C.	24	not	$\frac{1}{29}$	1	29	29		
,,	*b*	,,	,,	,,	,,	,,	0	0			
,,	*e*	,,	,,	,,	$\frac{1}{3}$	$\frac{1}{27}$	0	0	0		
,,	*f*	,,	,,	,,	,,	$\frac{1}{27}$	0	0		7	
,,	*i*	,,	,,	,,	$\frac{1}{5}$	$\frac{1}{28}$	0	0	0		
,,	*j*	,,	,,	,,	,,	,,	0	0			6
,,	*m*	,,	,,	,,	,,	$\frac{2}{28}$	0	0	0		
,,	*n*	,,	,,	,,	,,	,,	0	0			
,,	R_1	,,	,,	,,	not	$\frac{1}{27}$	0	0			
,,	R_2	,,	,,	,,	,,				0	0	
,,	S_1	,,	,,	,,	,,	$\frac{1}{27}$	0	0			
,,	S_2	,,	,,	,,	,,						
,,	*a*	,,	,,	48	,,	$\frac{1}{29}$	20	580	609		
,,	*b*	,,	,,	,,	,,	,,	22	638			
,,	*e*	,,	,,	,,	$\frac{1}{3}$	$\frac{1}{27}$	6	486	576		
,,	*f*	,,	,,	,,	,,	$\frac{1}{27}$	8	648		600	
,,	*i*	,,	,,	,,	$\frac{1}{5}$	$\frac{1}{28}$	7	980	910		
,,	*j*	,,	,,	,,	,,	,,	6	840			520
,,	*m*	,,	,,	,,	,,	$\frac{2}{28}$	3	210	315		
,,	*n*	,,	,,	,,	,,	,,	6	420			
,,	R_1	,,	,,	,,	not	$\frac{1}{27}$	24	648			
,,	R_2	,,	,,	,,	,,				440	440	
,,	S_1	,,	,,	,,	,,	$\frac{1}{27}$	27	432			
,,	S_2	,,	,,	,,	,,						
,,	*a*	,,	,,	72	,,	$\frac{1}{29}$	49	1,421	1,464	1,464	1,464
,,	*b*	,,	,,	,,	,,	,,	52	1,508			

Table S—*continued.*

Date.	Plate.	Age of water.	Temperature of incubation.	Time incubated.	Diluted or not.	Number of drops.	Colonies on plate.	Calculated number of bacteria per 1 c.c.	Averages.	Total average.	
Dec. 28, 1893				hours.		c.c.					
,,	*e*	$\frac{1}{3}$ hour at 8–10° C.	8—12° C.	,,	$\frac{1}{3}$	$\frac{1}{27}$	15	1,214	1,093	1,466	1,610
,,	*f*	,,	,,	,,	,,	$\frac{1}{27}$	12	972			
,,	*i*	,,	,,	,,	$\frac{1}{5}$	$\frac{1}{28}$	12	1,680	1,750		
,,	*j*	,,	,,	,,	,,	,,	13	1,820			
,,	*m*	,,	,,	,,	,,	$\frac{2}{28}$	21	1,470	1,155		
,,	*n*	,,	,,	,,	,,	,,	12	840			
,,	R_1	,,	,,	,,	not	$\frac{1}{27}$	70	1,890	1,755	1,755	
,,	R_2	,,	,,	,,	,,						
,,	S_1	,,	,,	,,	,,	$\frac{1}{27}$	60	1,620			
,,	S_2	,,	,,	,,	,,						
,,	*a*	,,	,,	96	,,	$\frac{1}{29}$	80	2,320	2,436	2,441	3,346
,,	*b*	,,	,,	,,	,,	,,	88	2,552			
,,	*e*	,,	,,	,,	$\frac{1}{3}$	$\frac{1}{27}$	31	2,510	2,470		
,,	*f*	,,	,,	,,	,,	$\frac{1}{27}$	30	2,430			
,,	*i*	,,	,,	,,	$\frac{1}{5}$	$\frac{1}{28}$	16	2,240	2,800		
,,	*j*	,,	,,	,,	,,	,,	24	3,360			
,,	*m*	,,	,,	,,	,,	$\frac{2}{28}$	31	2,170	2,060		
,,	*n*	,,	,,	,,	,,	,,	28	1,960			
,,	R_1	,,	,,	,,	not	$\frac{1}{27}$	173	4,671	4,252	4,252	
,,	R_2	,,	,,	,,	,,						
,,	S_1	,,	,,	,,	,,	$\frac{1}{27}$	142	3,834			
,,	S_2	,,	,,	,,	,,						

,,	*a*	,,	,,	120	,,	$\frac{1}{30}$	105	3,045	3,915	3,926	5,358
,,	*b*	,,	,,	,,	,,	,,	165	4,785			
,,	*e*	,,	,,	,,	$\frac{1}{3}$	$\frac{1}{27}$	51	4,132	3,645		
,,	*f*	,,	,,	,,	,,	$\frac{1}{27}$	39	3,159			
,,	*i*	,,	,,	,,	$\frac{1}{6}$	$\frac{1}{28}$	29	4,060	4,200		
,,	*j*	,,	,,	,,	,,	,,	31	4,340			
,,	*m*	,,	,,	,,	,,	$\frac{2}{28}$	56	4,040	3,945		
,,	*n*	,,	,,	,,	,,	,,	55	3,850			
,,	R_1	,,	,,	,,	not	$\frac{1}{27}$	277	7,479	6,790	6,790	
,,	R_2	,,	,,	,,	,,						
,,	S_1	,,	,,	,,	,,	,,	226	6,102			
,,	S_2	,,	,,	,,	,,						
,,	*a*	,,	,,	144	,,	$\frac{1}{30}$	182	4,278	5,329		
,,	*b*	,,	,,	,,	,,	,,	220	6,380			
,,	*e*	,,	,,	,,	$\frac{1}{3}$	$\frac{1}{27}$	70	5,670	5,548	6,481	8,856
,,	*f*	,,	,,	,,	,,	$\frac{1}{27}$	67	5,427			
,,	*i*	,,	,,	,,	$\frac{1}{6}$	$\frac{1}{28}$	60	8,400	7,490		
,,	*j*	,,	,,	,,	,,	,,	47	6,580			
,,	*m*	,,	,,	,,	$\frac{1}{6}$	$\frac{2}{28}$	93	6,510	6,405		
,,	*n*	,,	,,	,,	,,	,,	90	6,300			
,,	R_1	,,	,,	,,	not	$\frac{1}{27}$	403	10,881	11,232	11,232	
,,	R_2	,,	,,	,,	,,						
,,	S_1	,,	,,	,,	,,	$\frac{1}{27}$	429	11,583			
,,	S_2	,,	,,	,,	,,						
,,	*a*	,,	,,	16	,,	$\frac{1}{30}$	293	8,497	10,254	9,978	29,385
,,	*b*	,,	,,	,,	,,	,,	414	12,006			
,,	*e*	,,	,,	,,	$\frac{1}{3}$	$\frac{1}{27}$	119	9,638	10,758		
,,	*f*	,,	,,	,,	,,	$\frac{1}{27}$	148	11,988			
,,	*i*	,,	,,	,,	$\frac{1}{6}$	$\frac{1}{28}$	85	11,900	10,010		
,,	*j*	,,	,,	,,	,,	,,	58	8,120			
,,	*m*	,,	,,	,,	,,	$\frac{2}{28}$	136	9,520	8,890		
,,	*n*	,,	,,	,,	,,	,,	118	8,260			
,,	R_1	,,	,,	,,	not	$\frac{1}{27}$	650	17,550	18,859	18,859	
,,	R_2	,,	,,	,,	,,						
,,	S_1	,,	,,	,,	,,	$\frac{1}{27}$	747	20,169			
,,	S_2	,,	,,	,,	,,						

Table S—*continued.*

Date.	Plate.	Age of water.	Temperature of incubation.	Time incubated.	Diluted or not.	Number of drops.	Colonies on plate.	Calculated number of bacteria per 1 c.c.	Averages.	Total average.	
				hours.		c.c.					
Dec. 28, 1893	*a*	$\frac{1}{4}$ hour at 8–10° C.	8—12° C.	192	not	$\frac{1}{29}$	312	9,048	10,614	11,017	12,610
"	*b*	"	"	"	"	"	420	12,180			
"	*c*	"	"	"	$\frac{1}{3}$	$\frac{1}{27}$	125	10,125	11,056		
"	*f*	"	"	"	"	"	148	11,988			
"	*i*	"	"	"	$\frac{1}{5}$	$\frac{1}{28}$	102	14,280	11,550		
"	*j*	"	"	"	"	"	63	8,820			
"	*m*	"	"	"	"	$\frac{2}{28}$	136	9,520	10,850		
"	*n*	"	"	"	"	"	174	12,180			
"	R_1	"	"	"	not	$\frac{1}{27}$	650	17,550	18,981	18,981	
"	R_2	"	"	"	"						
"	S_1	"	"	"	"	$\frac{1}{27}$	756	20,412			
"	S_2	"	"	"	"						
"	*a*	"	"	216	"	$\frac{1}{29}$	425	12,325	13,876	14,808	15,954
"	*b*	"	"	"	"	"	532	15,428			
"	*c*	"	"	"	$\frac{1}{3}$	$\frac{1}{27}$	187	15,141	16,561		
"	*f*	"	"	"	"	"	222	17,982			
"	*i*	"	"	"	$\frac{1}{5}$	$\frac{1}{28}$	105	14,700	11,900		
"	*j*	"	"	"	"	"	65	9,100			
"	*m*	"	"	"	"	$\frac{2}{28}$	193	13,510	14,875		
"	*n*	"	"	"	"	"	282	16,240			
"	R_1	"	"	"	not	$\frac{1}{27}$	781	21,087	22,410	22,410	
"	R_2	"	"	"	"						
"	S_1	"	"	"	"	$\frac{1}{27}$	842	22,734			
"	S_2	"	"	"	"						

,,	*a*	,,	,,	240	,,	$\frac{1}{26}$	400	13,340	14,471	15,146	16,714
,,	*b*	,,	,,	,,	,,	,,	538	15,602			
,,	*e*	,,	,,	,,	½	$\frac{1}{27}$	195	15,795	16,888		
,,	*f*	,,	,,	,,	,,	,,	222	17,982			
,,	*i*	,,	,,	,,	½	$\frac{1}{28}$	128	17,920	13,860		
,,	*j*	,,	,,	,,	,,	,,	70	9,800			
,,	*m*	,,	,,	,,	,,	$\frac{2}{28}$	200	14,000	15,365		
,,	*n*	,,	,,	,,	,,	,,	239	16,730			
,,	R_1	,,	,,	,,	not	$\frac{1}{27}$	794	21,438	22,986	22,986	
,,	R_2	,,	,,	,,	,,						
,,	S_1	,,	,,	,,	,,	$\frac{1}{27}$	905	24,435			
,,	S_2	,,	,,	,,	,,						
,,	*c*	,,	18—20° C.	24	not	$\frac{1}{26}$	6	174	188	304	269
,,	*d*	,,	,,	,,	,,	,,	7	203			
,,	*g*	,,	,,	,,	½	$\frac{1}{27}$	5	405	324		
,,	*h*	,,	,,	,,	,,	,,	8	243			
,,	*k*	,,	,,	,,	½	$\frac{1}{28}$	2	280	420		
,,	*l*	,,	,,	,,	,,	,,	4	560			
,,	*o*	,,	,,	,,	,,	$\frac{2}{28}$	5	350	280		
,,	*p*	,,	,,	,,	,,	,,	8	210			
,,	T_1	,,	,,	,,	not	$\frac{1}{27}$	5	135	135		
,,	T_2	,,	,,	,,	,,						
,,	*c*	,,	,,	48	,,	$\frac{1}{26}$	88	2,552	2,697	2,302	2,597
,,	*d*	,,	,,	,,	,,	,,	98	2,843			
,,	*g*	,,	,,	,,	½	$\frac{1}{27}$	26	2,106	2,065		
,,	*h*	,,	,,	,,	,,	,,	25	2,025			
,,	*k*	,,	,,	,,	½	$\frac{1}{28}$	10	1,400	1,540		
,,	*l*	,,	,,	,,	,,	,,	12	1,680			
,,	*o*	,,	,,	,,	,,	$\frac{2}{28}$	41	2,870	2,905		
,,	*p*	,,	,,	,,	,,	,,	42	2,910			
,,	T_1	,,	,,	,,	not	$\frac{1}{27}$	140	3,780	3,780		
,,	T_2	,,	,,	,,	,,						
,,	*c*	,,	,,	72	,,	$\frac{1}{26}$	230	6,670	7,250	6,442	7,881
,,	*d*	,,	,,	,,	,,	,,	270	7,830			
,,	*g*	,,	,,	,,	½	$\frac{1}{27}$	78	6,318	5,467		
,,	*h*	,,	,,	,,	,,	,,	57	4,617			
,,	*k*	,,	,,	,,	½	$\frac{1}{28}$	41	5,740	6,160		
,,	*l*	,,	,,	,,	,,	,,	47	6.580			
,,	*o*	,,	,,	,,	,,	$\frac{2}{28}$	113	7,910	8,890		
,,	*p*	,,	,,	,,	,,	,,	141	9,870			
,,	T_1	,,	,,	,,	not	$\frac{1}{27}$	431	11,637	11,637		
,,	T_2	,,	,,	,,	,,						

Table S—*continued.*

Date.	Plate.	Age of water.	Temperature of incubation.	Time incubated.	Diluted or not.	Number of drops.	Colonies on plate.	Calculated number of bacteria per 1 c.c.	Averages.	Total average.	
				hours.		c.c.					
Dec. 28, 1893	*c*	½ hour at 8—10° C.	18—20° C.	96	not	$\frac{1}{29}$	312	9,048	9,642	10,182	11,434
"	*d*	"	"	"	"	"	353	10,237			
"	*g*	"	"	"	$\frac{1}{3}$	$\frac{1}{27}$	78	6,318	6,763		
"	*h*	"	"	"	"	"	89	7,209			
"	*k*	"	"	"	$\frac{1}{5}$	$\frac{1}{28}$	72	10,080	9,940		
"	*l*	"	"	"	"	"	70	9,800			
"	*o*	"	"	"	"	$\frac{2}{28}$	201	14,070	14,385		
"	*p*	"	"	"	"	"	210	14,700			
"	T_1	"	"	"	not	$\frac{1}{27}$	609	16,443	16,443		
"	T_2	"	"	"	"						
"	*c*	"	"	120	"	$\frac{1}{29}$	Liquid	..	12,006	13,072	13,746
"	*d*	"	"	"	"	"	414	12,006			
"	*g*	"	"	"	$\frac{1}{3}$	$\frac{1}{27}$	Liquid	..	10,044		
"	*h*	"	"	"	"	"	124	10,044			
"	*k*	"	"	"	$\frac{1}{5}$	$\frac{1}{28}$	98	13,720	13,370		
"	*l*	"	"	"	"	"	93	13,020			
"	*o*	"	"	"	"	$\frac{2}{28}$	222	15,540	16,870		
"	*p*	"	"	"	"	"	260	18,200			
"	T_1	"	"	"	not	$\frac{1}{27}$	609	16,443	16,443		
"	T_2	"	"	"	"						
"	*d*	"	"	144	"	$\frac{1}{29}$	Liquid	..	12,006	14,804	15,132
"	*h*	"	"	"	$\frac{1}{3}$	$\frac{1}{27}$	130	10,530	10,530		
"	*k*	"	"	"	$\frac{1}{5}$	$\frac{1}{28}$	137	19,180	16,730		
"	*l*	"	"	"	"	"	102	14,280			
"	*o*	"	"	"	"	$\frac{2}{28}$	225	15,750	19,950		
"	*p*	"	"	"	"	"	345	24,150			
"	T_1	"	"	"	not	$\frac{1}{27}$	Liquid	..	16,443		
"	T_2	"	"	"	"						
"	*h*	"	"	168	$\frac{1}{3}$	"	130	10,530	10,530	16,331	16,331
"	*k*	"	"	"	$\frac{1}{5}$	$\frac{1}{28}$	160	22,400	18,340		
"	*l*	"	"	"	"	"	102	14,280			
"	*o*	"	"	"	"	$\frac{2}{28}$	225	15,750	20,125		
"	*p*	"	"	"	"	"	350	24,500			

In the following plates I have plotted the curves obtained from the means of the foregoing illustrative series of analyses. They bring out very clearly the differences in numbers referred to, and I am strongly of opinion that much valuable information would result from a systematic series of monthly analyses of the Thames water conducted along these lines.

I by no means pretend that the numbers themselves are of absolute value, any more than are those obtained by the ordinary methods of counting averages; but I do think that the selected cases suggest possible lines of departure for the systematic bacteriological analysis of such a river as the Thames, if a sufficient number of other data were taken in at the same time. These data should include at least the following: (1) the temperature of the river, (2) the amount of sunshine, (3) the organic analysis of the water, (4) the rainfall.

I am perfectly alive to the incompleteness of the above analyses in these respects, and they are only intended to show what I think should be done by a competent staff of assistants, if any attempt is made at a thorough investigation of the bacteriology of the Thames; and the same applies to any other water.

The particular object of the above analyses was to test the view that the actual number of bacteria present in winter is less than that in summer, and they strongly confirm that; and if we remember that this was so in 1893 in spite of (1) the river being lower in August, and therefore more concentrated as a food liquid, (2) the temperature being higher, and therefore more favourable to bacterial growth, it seems at least highly probable that the diminution in the bacteria is largely due to the increased insolation.

Nor is this all (though I defer the fuller consideration of this point) that my analyses suggest. I find very distinct evidence that the bacteria in the summer water are many of them *enfeebled* forms, suggesting a distinct inhibition or weakening of their powers of growth. In some cases it is certain that forms obtained in August, and which afterwards turned out to be identical with forms found in the winter, at first grew so feebly that their characters on the plates led one to put them down as distinct species or varieties.

I have given some experimental evidence bearing on this, and going to prove that it is due to the action of light on these forms. The matter is a very complex one, and I must refrain from further discussion of it until all the forms isolated during the year are worked out; but it is worth while, I think, to draw the attention of investigators to the matter. In one or two cases, at least, there is no question that exposure to light *does* so affect the germination and growth of the bacteria, that the resulting colonies depart widely from the normal in many of their characters.

I have in hand a large number of experimental results obtained

from the detailed investigation of a single species and the measurement of the growth of a single filament (watched continuously under the microscope) under different conditions of exposure; they have been incidentally referred to in my lecture at the Royal Institution, in May, 1894, and I shall hope to bring them before the Royal Society shortly. They prove still more conclusively at least the main point, that exposure to sunlight does materially affect the rate and manner of growth, &c., of a bacterium rodlet or filament, as well as the germinal power of the spores.

As regards further details on the action of light on the spores and bacilli, I may refer the Committee to my memoir, now in the hands of the Royal Society, an abstract of which appeared in the 'Proceedings,' vol. 54, p. 472.

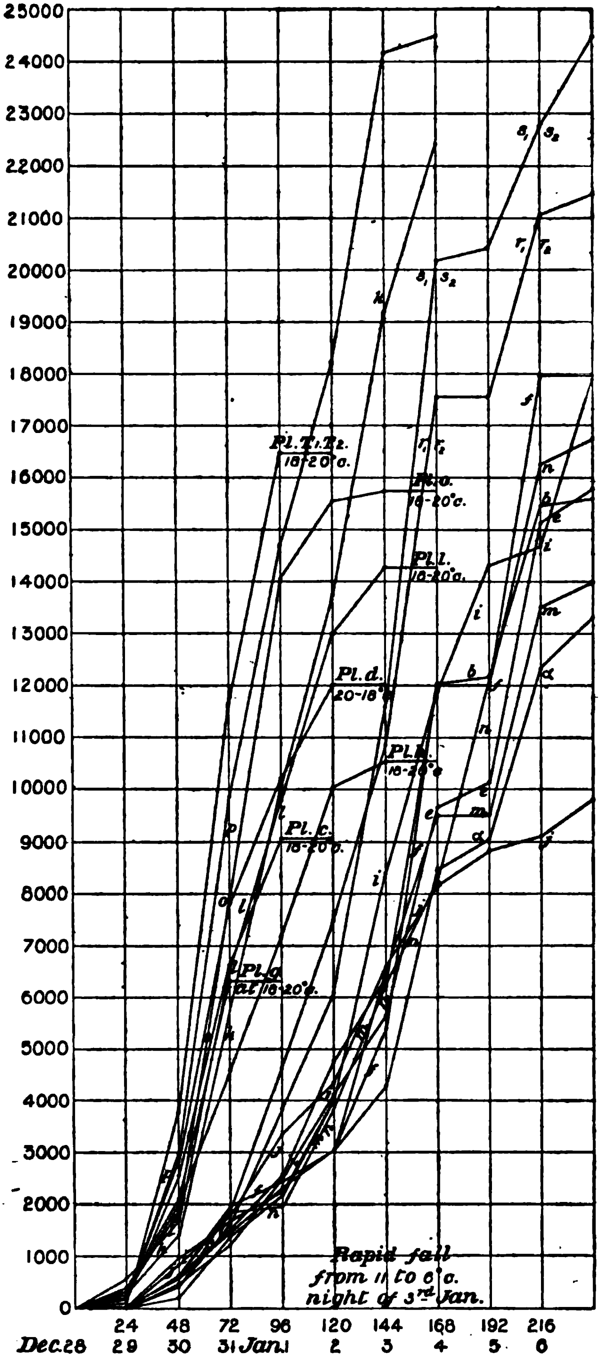


DIAGRAM IV.—Curves of Table S, showing total number of bacteria at 10—12° and 18—20° C. in December. Abscissæ = hours of incubation; ordinates = numbers of bacteria per 1 c.c.

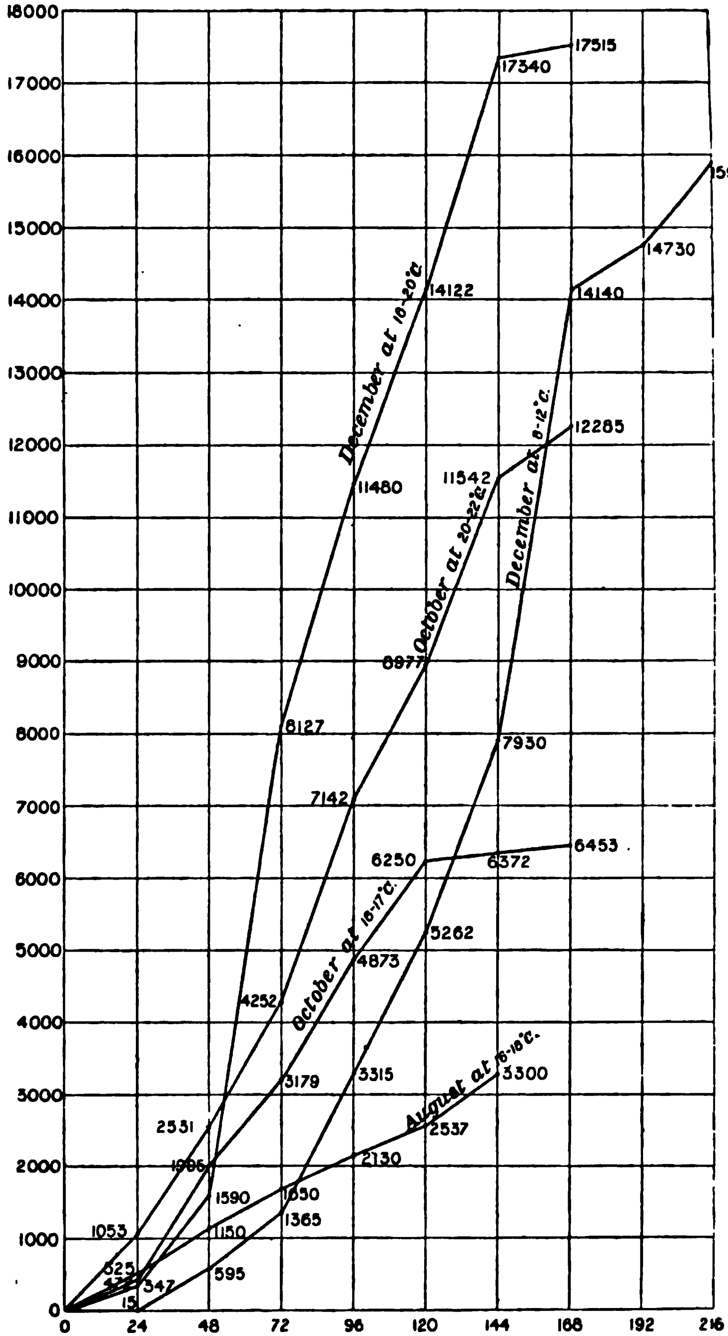


Diagram V.—Means of the foregoing curves, showing average numbers of bacteria for August, October, and December.

PART II.

"The Behaviour of the Typhoid Bacillus and of the *Bacillus Coli Communis* in Potable Water." By PERCY FRANKLAND, Ph.D., B.Sc. (Lond.), F.R.S., Professor of Chemistry in Mason College, Birmingham, assisted by J. R. APPLEYARD, F.C.S.

It has already been pointed out in the previous Reports that the only two zymotic diseases which have been conclusively proved to be communicable, and frequently communicated by drinking water, are Asiatic cholera and typhoid fever. The behaviour in water of the particular micro-organisms which are almost universally credited with the power of exciting these specific maladies is obviously, therefore, one of the most interesting and important questions in the whole domain of the hygiene of water supply.

Inasmuch as Asiatic cholera is, fortunately, only an occasional visitor of these islands, or, indeed, of the continent of Europe, the investigation of this question with regard to this disease is certainly of less immediate consequence than is its investigation with regard to typhoid fever, which we have always with us, and which claims such a large number of victims annually from amongst our population.

In the present Report I have, therefore, endeavoured on the one hand to summarise briefly what has already been done by others towards the elucidation of this subject of the behaviour of the typhoid bacillus in water, whilst on the other hand I have recorded those experiments which I have myself conducted with a view to extending the knowledge of this matter in general, and in respect to the conditions of water-supply pertaining to this country in particular.

Our information concerning the behaviour of the typhoid bacillus in water is of essentially two different kinds; firstly, this bacillus has on a number of occasions been discovered with more or less certainty in waters which were actually being used for domestic purposes, and to which it had therefore gained access unintentionally and in the natural course of events; secondly, the bacillus has been purposely introduced into various waters in which its subsequent fate has then been traced by experimental observation. It will be convenient to consider these two different kinds of information separately.

1. *Discovery of the Typhoid Bacillus in Natural Waters.*

Inasmuch as the communicability of typhoid fever by drinking water has been long recognised as a cardinal principle of modern hygiene, it was only natural that the discovery of the specific micro-organism of this disease by Eberth should have been soon followed by strenuous efforts to discover the Eberth-bacillus in potable water; it was not until about six years afterwards, however, that successful attempts in this direction were announced.

The first investigator who claimed to have discovered Eberth-Gaffky's bacillus in water was Moers ("Die Brunnen der Stadt Mühlheim a. Rhein vom bakteriologischem Standpunkte aus betrachtet," 'Ergänzungsh. zum Centralblatt f. allgem. Gesundheitspflege,' vol. ii, 1886, p. 144), who isolated the bacillus from a contaminated well supplying drinking water to a number of people amongst whom many cases of typhoid fever had occurred.

This discovery was soon followed by a similar announcement from Michael ("Typhusbacillen im Trinkwasser," 'Fortschritte d. Medicin,' vol. iv, 1886, p. 353), in Dresden, who claimed to have isolated the bacillus from a well-water which was suspected of being the source of an outbreak of typhoid which declared itself at the end of the year 1885.

Dreyfus-Brisac and Widal ("Epidémie de Famille de Fièvre typhoïde," 'Gaz. hebdom.,' 1886, No. 45) again detected the bacilli in the polluted water of a well at Ménilmontant, where typhoid fever had been prevalent for some months.

The typhoid bacillus has repeatedly been found in the water of the river Seine, thus Chantemesse and Widal ('Gaz. hebdom. de Méd. et de Chirurg.,' 1887, pp. 146—150; 'Centralbl. f. Bakteriol.,' vol. i, 1887, p. 682) discovered the bacillus no less than three times in this water during an outbreak of typhoid in Paris. Thoinot ('La Semaine Médicale,' 1887, No. 14, p. 135; 'Centralbl. f. Bakteriol.,' vol. ii, 1887, p. 39) also isolated typhoid bacilli from the Seine at Ivry, at a distance of but little more than twenty yards from the point where the water is abstracted for the occasional supply of Paris with drinking water. Again, Loir ("Recherche du Bacille typhique dans les Eaux d'Alimentation de la Ville de Paris," 'Annales de l'Institut Pasteur,' vol. i, p. 488) detected the typhoid bacillus in Seine water which was actually being distributed to a portion of Paris during the summer of 1887, owing to the scarcity of the Vanne water, which yields the supply under ordinary circumstances. Vincent ("Présence du Bacille typhique dans l'Eau de Seine pendant le Mois de Juillet, 1890," 'Annales de l'Institut Pasteur,' vol. iv, p. 772) again found the typhoid bacillus in the Seine water which was being similarly supplied to Paris during the summer of 1890.

Beumer ("Zur Aetiologie d. Typhus abdominalis," 'Deutsche medicinische Wochenschrift,' 1887, No. 28) was able to detect the typhoid bacillus in a well-water used for drinking purposes in a country place near Greifswald, where an outbreak of typhoid had occurred.

A similar discovery of the bacillus was made by Brouardel and Chantemesse ("Enquête sur les Causes de l'Epidémie de Fièvre typhoïde qui a régné à Clermont-Ferrand," 'Annales d'Hygiène publique et de Médecine légale,' vol. xvii, 1887, pp. 385—403; also 'Revue d'Hygiène,' vol. ix, p. 368) in the course of an investigation of an epidemic of typhoid which prevailed at Clermont-Ferrand, and in which neighbouring places using the same water supply were also involved.

Finkelnburg ("Ueber einen Befund von Typhusbacillen im Brunnenwasser," 'Centralbl. f. Bakteriol.,' vol. ix, p. 301) states that he isolated the typhoid bacillus from a well which had in all probability been contaminated with typhoid dejecta.

Henrijean ("Contribution à l'Etude du Rôle étiologique de l'Eau potable dans les Epidémies de Typhus," 'Annales de Micrographie,' vol. ii, p. 401) found typhoid bacilli in the drinking water of a Belgian village during an epidemic of typhoid fever.

Kamen ("Zum Nachweise der Typhusbacillen im Trinkwasser," 'Centralbl. f. Bakteriol.,' vol. xi, p. 32) detected typhoid bacilli in water supplying a Russian military garrison, amongst whom typhoid fever had broken out.

Péré ("Contribution à l'Etude des Eaux d'Alger," 'Annales de l'Institut Pasteur,' vol. v, p. 79) states that he was able to isolate the typhoid bacillus from drinking water in Algiers, where typhoid is endemic, occurring every year during the months of August, September, and October.

Martin states ("Présence du Bacille typhique dans les Eaux d'Alimentation de la Ville de Bordeaux," 'Revue sanit. de la Province,' 1891, No. 181, p. 93; 'Centralbl. f. Bakteriol, vol. xi, 1892, p. 413) that the typhoid bacillus was found by Pouchet in the public water supply of Bordeaux during an outbreak of typhoid in that city.

Fodor ("Die Beziehungen des Typhus zum Trinkwasser," 'Centralbl. f. Bakteriol.,' vol. xi, 1892, p. 121), in a paper read at the International Hygienic Congress held in London in 1891, describes an outbreak of typhoid fever at Budapest, during which he succeeded in detecting the typhoid bacillus no less than five times in the public water-supply. It was afterwards ascertained that the waste water from a laundry attached to the hospital gained direct access to the principal water main in the town.

Kowalski ("Ueber bakteriologische Wasseruntersuchungen,"

'Wiener klinische Wochenschrift,' 1888; 'Centralbl. f. Bakteriol.' vol. iv, 1888, p. 467) states that out of 2000 samples of water which he had examined bacteriologically, there were only five in which he was able to detect typhoid bacilli.

From the above it will be seen that many investigators, since the year 1886, claim to have discovered the typhoid bacillus in potable waters, but, in the majority of cases, these discoveries, especially the earlier ones, must be accepted with considerable reserve, as, until recently, it was customary to rely for the identification of the typhoid bacillus on altogether insufficient data, as it only gradually became understood that there are several other forms presenting the closest points of resemblance in their morphological characters, both micro and macroscopic, to the typhoid bacillus, with which they are not unfrequently associated, and, moreover, some of these simulatory forms are of very frequent occurrence in natural waters. Indeed, even at the present day, the identification of particular forms or "species" of bacteria is in a transitional and highly unsatisfactory state, as it is daily becoming clearer that the characters, both morphological and physiological, of one and the same micro-organism are often liable to the profoundest modifications through changes of environment and other causes, whilst there are almost daily being discovered in nature new forms which differ only from already well-known forms or "species" in what appear to be the most minute, trifling, and insignificant particulars.

Under these circumstances, it is practically certain that some of the bacilli discovered in water, and believed to have been typhoid bacilli, must, in reality, have been only forms closely simulating the more striking characters of the typhoid bacillus. On the other hand, it is equally certain that a great many waters which have been submitted to examination for typhoid bacilli may have contained them without their being discovered, for, as will be pointed out later, the ordinary method pursued in the bacteriological examination of water, in which a few drops, or at most a cubic centimetre or two, of the water is submitted to plate cultivation, can only, under the most exceptional circumstances, lead to the detection of typhoid bacilli.

Thus, whilst there is considerable evidence that the typhoid bacilli have been found on a number of occasions in waters which had been convicted of distributing typhoid amongst their consumers, the failure to discover these bacilli in other waters equally guilty need excite no surprise when the very imperfect methods of examination which are commonly employed for their discovery are taken into consideration.

In connection with the above discoveries of typhoid bacilli in potable water, I will only at this stage further remark that, in by far the majority of cases, the waters accused of containing these bacilli

were well waters, and, as is well known, it is just this kind of water which has most frequently been conclusively convicted of distributing typhoid fever.

The above discoveries of the typhoid bacillus in natural waters have, in almost all cases, been made not by means of the ordinary method of plate cultivation, which affords little or no chance of a few typhoid bacilli being discovered amongst a host of common water bacteria, the colonies of which generally grow with great rapidity and not unfrequently cause rapid liquefaction of the gelatine, but by special methods of treatment which have been devised to oppose the proliferation of the water bacteria whilst not materially interfering with the growth and multiplication of the typhoid bacilli, the latter thus acquiring a large numerical preponderance, if not entirely excluding the other forms present in the water.

Unfortunately, these conditions which foster the growth of the typhoid bacillus to the exclusion of the ordinary water bacteria are equally propitious to other microbes which are invariably associated with the typhoid bacillus, and which, in fact, resemble it so closely, especially in morphological characters, that they may easily be mistaken for the typhoid bacillus, and it is this circumstance which causes so much doubt to attach more especially to the earlier of those alleged discoveries of the typhoid bacillus in potable water which are recorded above.

The particular micro-organism, which is especially liable to cause confusion in this respect, is the so-called *Bacillus coli communis*. This organism was described by Escherich, and is found regularly in the human intestinal tract and fæces, as well as in the excreta of other animals. It is regarded as identical with the *Bacillus neapolitanus* (Emmerich), and the "Fæces bacillus" described by Weisser, whilst by recent experiments I have shown that it is closely allied to, if not identical with, the *Bacillus ethacetosuccinicus* previously described by me. In all cases, therefore, in which water is supposed to have been infected with the dejecta of typhoid patients, the *B. coli communis* may be expected also to be present. In order, therefore, to ascertain definitely whether the typhoid bacillus is present in any given water, care must be taken that the *B. coli communis* is not mistaken for the former, and to guard against this it would be a desideratum to have some method which would, whilst revealing the presence of the typhoid bacillus, effectually eliminate or separate out its almost constant attendant, the *B. coli communis*. Unfortunately, this is a consummation which has not yet been realised in fact, for not only is the vitality of the *B. coli communis* in water, as will be shown below, superior to that exhibited by the typhoid bacillus, but in all attempts which have so far been made to suppress the vitality of other organisms, and yet permit of the

development of the latter, the *B. coli communis* has shown itself to be possessed of greater powers of resistance than the typhoid bacillus itself. Hence, although the addition of various chemical substances in suitable proportions may effectually destroy or retard the growth of other organisms, yet the *B. coli communis* survives and remains present along with the typhoid bacillus; indeed, in many cases it has been shown that such additions have actually destroyed the typhoid bacillus, and left the *B. coli communis* alone master of the field. It is true that, growing in artificial cultures side by side, there are certain differences observable between these two organisms, for the *B. coli communis* grows more luxuriantly in the various culture-media employed than does the typhoid bacillus, or to use the expressive language of a French observer, the *B. coli communis* grows as though it were *well*, and the typhoid bacillus as though it were *ill;* but yet on the gelatine plates of each there are nearly always colonies which are indistinguishable from those of the other, whilst even in the potato-cultures, which used to be regarded as the crucial test for the typhoid bacillus, the *B. coli communis* may and does exhibit, under certain conditions, growths which resemble in every respect those produced by the typhoid bacillus. In two media, however, as has been pointed out by Dunbar ("Ueber den Typhusbacillus und den Bacillus Coli Communis," 'Zeitsch f. Hygiene,' 1892, 491), a marked difference is found in the behaviour of these two organisms. Thus, when inoculated into sterile milk, the typhoid bacillus renders the liquid slightly acid, but *never* causes its coagulation; the *B. coli communis*, on the other hand, at the temperature of the body coagulates the milk in from 24—48 hours, rendering it at the same time strongly acid. Again, when grown in sterile fluid meat extract or broth, the *B. coli communis* at 37° C. produces, in the course of from 3—12 hours, a quantity of gas (consisting of hydrogen and carbonic anhydride), whilst no formation of gas has, under the same conditions, ever been observed in the case of the typheid bacillus.

This latter mode of distinguishing between the *B. coli communis* and the typhoid bacillus I have reduced to the following extremely simple and handy form suitable for their rapid differentiation:—The organism under investigation is inoculated into a test-tube containing *ordinary gelatine peptone* in a melted state, the latter is shaken to distribute the bacilli throughout the liquid, which is then allowed to solidify and maintained at the ordinary temperature (18—20° C.). The tube, if it contains the *B. coli communis*, will invariably, after 12—48 hours, exhibit numerous conspicuous gas bubbles distributed through the solid medium, whilst no such bubbles make their appearance in similar tubes containing the typhoid bacillus. The test possibly depends upon the meat extract containing sufficient dextrose (derived from the *post-mortem* transformation of the glycogen

in the blood) for a visible fermentation by the *B. coli communis* to take place. The bubbles of gas are certainly independent of any ingredients present in either the gelatine or in the peptone, for I have found them to form also in agar-agar-peptone, and also in meat-extract gelatine to which no peptone had been added.* The great convenience of the test depends upon its involving only the use of a medium which must invariably be at hand at all times in every bacteriological laboratory, and also on its dispensing with the use of an incubating temperature, whilst it has the further advantage over Dunbar's original broth-bubble test that the bubbles of gas being fixed in the solid gelatine, the tubes can be examined at leisure even days or weeks after inoculation, whilst with the broth-bubble test, if the tubes are not examined at the right time, the fermentation may have ceased; besides, in the broth, of course, the bubbles are not nearly so conspicuous. Extensive use has been made of this method during the present investigation, and for rapidly and certainly distinguishing between the typhoid bacillus and the *B. coli communis* I have found it unequalled; on the other hand, it must be borne in mind that it does not serve to distinguish between the *B. coli communis* and many other fermenting organisms.

A further but less certain distinction which should also be employed for differentiating between the typhoid bacillus and the *B. coli communis* is the so-called *indol-reaction.* This test is best applied in the following manner, as recommended by Kitasato :—

To 10 c.c. of the culture in ordinary alkaline peptone-broth of the organism under examination, and which has been growing for twenty-four hours in the incubator, add 1 c.c. of a solution of potassium or sodium nitrite (containing 0·02 gram in 100 c.c.) and then a few drops of concentrated sulphuric acid. *If indol is present, a rose to deep-red coloration is produced,* depending on the interaction of nitrous acid with indol to form nitrosoindol nitrate which is of red colour. On applying this test to the *B. coli communis* an indol-reaction should be obtained, whilst the typhoid bacillus gives invariably a negative result. In practice I have found it advisable not to apply the indol-test until the broth-culture has been forty-eight hours in the incubator.

Although the *B. coli communis* is generally supposed to give the indol-reaction, this character would appear not to be so constant as is commonly imagined. In my own experiments I have known *one and the same culture-series* of the *B. coli communis* not to give the indol-reaction at one time, and yet subsequently to become possessed of this power, although I have not been able to determine the cause which leads to the loss of the indol-producing capacity. Thinking

* Dunbar found also that no bubbles were formed in a solution of peptone without meat extract.

that it might possibly be due to the growth of the bacillus having become enfeebled, I tried growing it under unfavourable conditions, viz., in phenol-broth, in which I left the bacillus for months without transplanting, but even by this severe treatment I found no diminution in the indol-producing power. The absence of indol-production by the *B. coli communis* has also been noticed by Dunbar, who in his exhaustive memoir (*loc. cit.*) compares the behaviour of a culture of the typhoid bacillus with a culture of the *B. coli communis*, and found that neither bacilli gave the indol-reaction. It is obvious, therefore, that indol production is not a necessary attribute of the *B. coli communis*, and that too much reliance must not be placed on it as a means of distinguishing between the typhoid bacillus and the *B. coli communis*.

As convenient, for purposes of reference, I have collected in the following two tables the principal characters of these two bacilli:—

Typhoid Bacillus.

(*Bacillus typhi abdominalis.*)

Authority.—*Eberth*, 'Virchow's Archiv,' vol. 81, 1880; also *ibid.*, vol., 83, 1881. Gaffky, "Zur Aetiologie des Abdominaltyphus," 'Mittheilungen a. d. Kaiserlichen Gesundheitsamte,' vol. 2, 1884, p. 372.

Where Found.—In the blood, urine, fæces, as well as in the organs of typhoid patients. Found by numerous investigators in water.

Microscopic Appearance.—A short, plump bacillus about three times as long as broad, with rounded ends. It occurs in the tissues usually singly, but in artificial cultures it grows frequently into long threads. It is very motile and is provided with numerous cilia, which are attached to both the sides and ends of the bacillus. To stain the cilia add 22 drops of caustic soda to 16 c.c. of the mordant (Loeffler). It is not stained by Gram's method, and stains less readily with aqueous aniline solutions than most bacteria. Günther recommends heating the cover-glass, after the dye has been poured on it, for a few seconds until it begins to steam, and then washing off the stain as usual. It does not form spores.

Cultures: Gelatine Plates.—The colonies on the surface form large spreading greyish-white iridescent expansions with jagged and irregular edge. Under a low power they exhibit a brownish shimmer and a characteristic woven structure. The depth colonies are darker, with regular edge, and are covered with delicate irregular lines. No liquefaction takes place.

Gelatine Tubes.—Grows chiefly on the surface, producing a delicate greyish-white iridescent expansion with irregular edge.

Agar-agar.—Forms a greyish-white moist expansion.

Potatoes.—Produces an almost invisible greyish-white growth after forty-eight hours, but on touching the moist-looking surface with the needle a tough resistant pellicle is found. On some potatoes, however, its growth is more apparent, so that the above is not the only appearance to which it may give rise.

Blood Serum.—Produces a milk-white expansion restricted to the path of the needle.

Broth.—Renders it turbid.

Milk.—Grows abundantly, rendering it slightly acid. No coagulation takes place.

Remarks.—It grows best at about 37° C. Kitasato states that it produces no indol reaction. It produces sulphuretted hydrogen in iron-gelatine, the needle-track after from five to six days becoming intensely black in colour. In iron-agar, at from 33° to 34° C., this black colour appears at the end of 24 hours (Fromme). It produces sulphuretted hydrogen in broth with or without peptone; comparative tests made with the *B. coli communis* revealed no difference either in the degree of the reaction (as shown by the lead-paper test) or in the rapidity with which it took place in the case of the two organisms. The typhoid bacillus never produces gas in any artificial media. It is destroyed when heated for ten minutes at 60° C.

Injection into the aural vein of rabbits causes death in 24—28 hours (Fraenkel and Simmonds), guinea-pigs into which the cultures are introduced by the mouth, as described for cholera, are also killed (Seitz). Opinion is, however, still divided as to whether death is due to mere intoxication by the bacterial products present in the cultures, or to actual multiplication of the bacillus within the animal. In this connection, see Petruschky ('Zeitsch. f. Hygiene,' vol. 12, 1892, p. 269).

Bacillus Coli Communis.

Authority.—Escherich, 'Fortschritte der Medicin,' vol. 3, 1885, Nos. 16 and 17. Also Dunbar, "Ueber den Typhus-bacillus und den Bacillus Coli-communis," 'Zeitschrift f. Hygiene,' vol. 12, 1892, p. 485. Also Luksch, "Zur Differenzialdiagnose des Bacillus typhi abdominalis (Eberth) und des Bacterium Coli-commune (Escherich)," 'Centralblatt f. Bakteriologie,' vol. 12, 1892, p. 427.

Where Found—In the intestinal tract of man and animals. Found often in water by numerous investigators, and frequently mistaken for the typhoid bacillus.

Microscopic Appearance.—The typical form is a short bacillus 0·4 μ broad and 2 to 3 μ long; it is, however, very variable, oval individuals and forms resembling cocci being also found. It exists chiefly as a double bacillus arranged in groups. It is slightly motile, and is provided with 1 to 3 cilia, whilst the typhoid bacillus has 8 to

12 cilia (Luksch). Nicolle and Morax mention that the *coli* bacillus has invariably fewer cilia than the typhoid, that whereas the former rarely possesses more than six, the latter usually exhibits ten to twelve, whilst the cilia of the former are also far more fragile ('Annales de l'Institut Pasteur,' vol. 12, 1893, p. 561). It does not form spores.

Cultures: Gelatine Plates.—Forms round, and very often oval, smooth-rimmed granular colonies in the depth, which later become yellowish-brown in colour. On the surface it forms flat, irregular, pale white expansions, which under a low power exhibit a furrowed appearance due to the unequal thickness of the colony in its different parts. The colony also presents a distinctly wavy lineal structure parallel to the periphery. No liquefaction ensues.

Gelatine Tubes.—Grows somwhat abundantly in the depth, producing small white pin-head colonies, whilst on the surface it forms an expansion resembling the growth on gelatine plates.

Agar-agar.—Grows abundantly on the surface, producing a dirty-white, faintly shining expansion.

Blood Serum.—Forms a milk-white expansion.

Potatoes.—Produces a slimy yellow expansion on some potatoes, on others grey-white, whilst in some cases it resembles the typhoid bacillus in being hardly visible.

Broth.—Renders it turbid.

Milk.—Renders it acid, and at 37° C. coagulates it in from twenty-four to forty-eight hours.

Remarks.—Both cultures of twenty-four hours' age generally exhibit considerable evolution of gas; ordinary gelatine or agar stab-cultures also generally exhibit bubbles of gas in the solid medium. Such bubbles can invariably be obtained by inoculating into ordinary melted gelatine, which is afterwards allowed to solidify (Percy Frankland). The addition of dextrose to the gelatine is quite unnecessary for this purpose. Exhibits indol reaction after twenty-four to forty-eight hours' culture in peptone broth.

Is capable of exhibiting very different degrees of pathogeneity according to its origin, cultures made from diseased tissues in which it is present on being intraperitoneally inoculated into rabbits cause peritonitis, and the bacilli are found in pure culture in the heart's blood. (Alex. Fraenkel, 'Wiener klin. Wochenschr.,' 1891, Nos. 13—15.)

2. *Behaviour of Typhoid Bacilli experimentally introduced into Potable Water.*

The second kind of information concerning the behaviour of the typhoid bacillus in water has, as already mentioned, been gained by

direct experiment, *i.e.*, by purposely introducing the bacillus into water, and in this manner the conditions which are favourable and unfavourable to the vitality in water of the typhoid bacillus can, of course, be much more readily ascertained than by the study of such chance cases as those already enumerated above, in which the bacilli had, in the natural course of events, gained access to water.

I have below expressed in a tabular form the principal results of the numerous investigators who have already availed themselves of this method of experimenting on the behaviour of the typhoid bacillus in water.

From this table it will be seen that different observers ascribe very different degrees of vitality to the typhoid bacillus in water, nor is this to be wondered at when it is remembered that the typhoid bacilli introduced into the waters may have been possessed of very different degrees of initial vitality according to their age and previous history; whilst, secondly, the waters experimented with were, of course, not the same; thirdly, the amount of food-material introduced into the water along with the bacilli must have been subject to the very greatest variations, and again the temperatures and other conditions under which the infected waters were preserved were equally variable.

Thus, taking the experiments made with distilled water, in which, therefore, there is the most chance of the water having been of uniform quality, Braem found the introduced typhoid bacilli still alive after 188 days, whilst the longest duration of vitality in this medium observed by Hochstetter was five days, Meade Bolton, Slater, Straus and Dubarry, and Wolffhügel and Riedel giving periods intermediate between these two wide extremes. These discrepancies in the case of distilled water are doubtless to be accounted for partly by the difference in initial vitality possessed by the different typhoid bacilli employed and partly by the difference in the amount of culture-material imported into the distilled water along with the bacilli themselves, whilst the actual numbers in which the bacilli were introduced may also greatly influence the degree of longevity observed.

This wide divergence in the results obtained by previous observers would alone call for a reinvestigation of this subject with a view to ascertaining the longevity of the typhoid bacillus in definite types of British potable water, and taking more into consideration the exact chemical composition of the waters experimented with.

There is, however, another point arising out of the results arrived at by previous investigators which still more urgently demands re-investigation with a view to its confirmation, qualification, or direct contradiction, and this is the relatively far greater longevity of the typhoid bacillus in sterilised than in unsterilised potable water,

which has been affirmed more especially by Kraus and subsequently by Karlinski. This point is obviously of the very highest importance from a practical hygienic point of view, as it is with unsterile potable water that we are in practice alone concerned, and the duration of vitality ascribed to the typhoid bacillus in such water by both these observers is of very limited extent—not more than seven days.

The experiments of Kraus are so striking, and have attracted so much attention, that I will give them in more detail in the following table:—

Typhoid Bacillus.

Description of Water.	Number of Days after Inoculation when Examined.							
	1.	2.	3.	5.	7.	9.	20.	150.
	Number of Typhoid Bacilli found in 1 c.c. of Water.							
(1) Munich water supply (Mangfall)	57,960	50,400	15,680	9,000	0	0	0	0
(2) Well-water, Munich......	57,000	50,840	32,643	8,900	0	0	0	0
(3) " "	56,000	35,910	10,010	7,060	0	0	0	0
	Number of Water Bacteria found in 1 c.c. of Water.							
(1) Munich water supply (Mangfall)	0	0	0	80	288,000	400,000	970,000	1,080
(2) Well-water, Munich......	0	0	490	lost	300,000	427,000	innumerable	1,980
(3) " "	0	0	280	500	256,000	lost	456,000	1,050

These results indicate, therefore, that, on introducing the typhoid bacilli into the potable waters in question, which were almost naturally sterile, the typhoid bacilli promptly disappeared as soon as the water bacteria had undergone extensive multiplication, which had taken place in each of the three experiments by the seventh day after the importation of the typhoid bacilli.

These interesting experiments cannot, however, by the light of our present knowledge, be accepted without criticism, for there cannot be the slightest doubt that, when only the ordinary method of plate-cultivation is employed in such investigations on unsterilised water, the typhoid bacilli will be generally overlooked unless they are present in large numbers. Again, the attempts which have been made, both by Kraus and other experimenters, to count the typhoid colonies on plates containing such mixtures of colonies, and the numerical estimates given of the typhoid bacilli in such unsterilised waters, must be wholly illusory, for the number of typhoid colonies which develope what may be called a typical appearance (*i.e.*, one which enables them to be readily recognised with reasonable certainty) depends on a variety of different circumstances, amongst which may be mentioned the age of the plate, the extent to which

the colonies are crowded together on the plate, very probably, also, the nature of the other colonies on the plate, and certainly the degree of vitality possessed by the typhoid bacilli themselves. Thus, if the numerical estimate of the typhoid colonies on a plate is made by counting as such the characteristic surface expansion colonies only, the result must be entirely fallacious, as nothing is, in my experience, commoner than to find only a vanishing proportion of the total number of typhoid colonies, even on a pure plate, giving rise to these surface-expansions at all.

In looking for typhoid bacilli in such artificially-infected unsterile waters it is, in fact, necessary to employ special methods for their detection similar to those which, as already pointed out, had to be devised for the examination of natural waters for the typhoid bacillus, and it is only when such special methods have been employed with a negative result that the conclusion can be legitimately drawn that the typhoid bacillus is not present in the living state in the particular volume of water operated on.

In the present investigation, the uniform practice has been made of examining all unsterile waters by means of Parietti's method of phenol broth-culture (see description below) in order to ascertain the presence or absence of the typhoid bacillus or of the *B. coli communis.*

Method of Detecting the Typhoid Bacillus and Bacillus Coli Communis *in Unsterile Waters.*

It will not be necessary to describe the various methods which have been devised for discovering the presence of typhoid bacilli in water, but it will be sufficient to point out that these are nearly all based upon the fact that the typhoid bacillus is, in comparison with most bacteria, but little affected by small doses of either phenol or dilute acids, so that, by adding suitable quantities either of phenol alone or of phenol in conjunction with acid to the culture-media, the growth and multiplication of the typhoid bacillus is not materially interfered with, whilst the proliferation of most, if not of all, the water-bacteria is suppressed.

Of these various methods, the one which I selected for the purposes of this investigation was that devised by Parietti ("Metodo di ricerca del Bacillo del tifo nelle aque potabili," 'Rivista d' Igiene e Sanità pubblica,' 1890). This method, which consists in adding phenol along with hydrochloric acid in certain proportions to neutral broth is carried out as foilows :—

A series of test-tubes containing 10 c.c. of neutral broth, each receive from 3 to 9 drops of a solution having the following composition :—

Phenol	5	grams.
Hydrochloric acid (pure)	4	„
Distilled water................	100	„

(In practice, I generally employ some tubes to which 3 drops (= 0·25 c.c.) and others to which 5 drops (= 0·4 c.c.) of this solution have been added.)

To the tubes thus treated, from 1 drop to several cubic centimetres of the water under investigation are added, and, after thoroughly mixing the contents, the tubes are placed in the incubator at 37° C. As soon as the tubes become turbid (which in the initial presence of many typhoid bacilli will occur already in twenty-four hours, but if only few are present, may be postponed for forty-eight, seventy-two, or even more hours) they are submitted to ordinary plate cultivation in three dilutions, the second and third dilutions only being actually poured on to plates or into Petri dishes, whilst the first dilution gelatine-tube should be preserved to see if gas-bubbles develope in it.

The gelatine-plates thus prepared are frequently found to yield nothing but typhoid colonies, whilst in some cases the latter are mixed with the colonies of water-bacteria, and in some cases, again, there are only colonies of water-bacteria on the plates. In no case must it be concluded from the mere appearance of the colonies that typhoid is present, but the colonies must always be submitted to the further tests of

1. Microscopic examination.
2. Inoculation on to potatoes, and comparison of growth with that of simultaneous cultures of the typhoid bacillus on the same potatoes.
3. Inoculation into broth, and examination of the broth-culture after forty-eight hours' growth in the incubator at 37° C. for indol, which, if it is the typhoid bacillus, should be absent.
4. Inoculation into milk, which should not subsequently become coagulated on keeping for one week in the incubator.
5. Inoculation into a tube containing melted gelatine-peptone; on distributing the bacilli in this and then congealing the gelatine, no gas-bubbles should be formed on keeping at 18—20° C., whilst in the case of the *B. coli communis* bubbles will make their appearance in from twelve to forty-eight hours.

The *B. coli communis*, as already pointed out, is even less sensitive to phenol and acids than the typhoid bacillus. In the case of those unsterile waters infected with the *B. coli communis*, the above method was similarly employed for its detection.

The above outline will show that a systematic investigation of the behaviour of the typhoid bacillus in unsterilised waters is attended with considerable difficulties, and involves an enormous amount of

labour, which is, however, well worth bestowing in consideration of the very great importance attaching to the question at issue.

First Series of Experiments.

The Vitality of the Typhoid Bacillus and of the Bacillus Coli Communis *in Thames Water.*

As already indicated above, I have endeavoured to make these experiments as far as possible comparable with those previously conducted by me on the *B. anthracis* and its spores recorded in the Second Report.

The Thames water was collected by me personally from the river, close to the intake of the Grand Junction Waterworks, near Hampton, on May 4, 1893; this spot was selected as being in that region of the river from which the supply for London is abstracted.

This water was submitted to chemical analysis with the following results:—

Results of Analysis expressed in Parts per 100,000.

Total solid matters	26·80	The sample was turbid and free from poisonous metals.
Organic carbon by combustion	0·247	
" nitrogen by combustion	0·038	
Organic nitrogen (by Kjeldahl method)	0·041	
Ammonia (free)	0·013	
" (albuminoid)	0·016	
Oxygen consumed by organic matter	0·102	
Nitrogen as nitrates and nitrites	0·124	
Total combined nitrogen	0·173	
Chlorine	1·65	
Temporary hardness	13·8	
Permanent "	4·4	
Total "	18·2	

The analysis shows that the water is chemically a typical sample of Thames water as found in this part of the river at this season of the year.

In this series of experiments I proposed introducing the typhoid and coli bacilli respectively into (*a*) *Thames water in its natural and unsterile condition;* (*b*) *Thames water sterilised by filtration through porous porcelain;** (*c*) *Thames water sterilised by steam,*† and to com-

* The Chamberland filter (see 2nd Report, 'Roy. Soc. Proc.,' vol. liii, p. 183) was sterilised in the steamer on two successive days, 1½ litres of the Thames water being then passed through it immediately before infection.

† 1500 c.c. of the Thames water were placed in the steamer for two hours on each of three successive days.

pare the respective behaviour of the two organisms in these three different states of the Thames water.

The infection of the several waters was made on 11.5.1893, as follows:—

I. *Typhoid.*—Forty needle-loops were removed from the surface of an agar-culture of the typhoid bacillus which had been grown at 18—20° C. for fourteen days, great care being taken to carry as little as possible of the culture-material along with the growth. This growth was introduced into 50 c.c. of steam-sterilised Thames water and violently shaken for fifteen minutes in a sterile bottle to thoroughly disintegrate the masses. This water-dilution was then employed for the infection of the larger quantities of water as indicated below.

II. *B. coli.*—The water-dilution of the *B. coli communis* was made in exactly the same way as that of the typhoid bacillus described above, excepting that only twenty-five needle-loops of the growth were taken, as owing to its greater thickness it was detachable in larger masses from the surface of the agar. The culture employed was of just the same age (fourteen days), and had been grown at the same temperature (18—20° C.) as the typhoid bacillus.

With the water-dilutions thus prepared the following infections were made:—

Typhoid bacillus.	*B. coli communis.*
Unfiltered Thames Water.	
2000 c.c. received 8 c.c. of the water dilution.	1000 c.c. received 3 c.c. of the water dilution.
Porcelain-filtered Thames Water.	
750 c.c. received 3 c.c. of the water dilution.	750 c.c. received 2 c.c. of the water dilution.
Steamed Thames Water.	
750 c.c. received 3 c.c. of the water dilution.	750 c.c. received 2 c.c. of the water dilution.

The waters thus infected were each subdivided amongst a number of smaller sterilised flasks plugged with sterile cotton-wool, and these were respectively placed in the incubator or refrigerator, according as they were to be exposed to a winter or a summer temperature. Thus:

INFECTED.

Typhoid.	*B. Coli.*
Unfiltered Thames Water.	
3 flasks incubator.	3 flasks incubator.
3 „ refrigerator.	3 „ refrigerator.

Porcelain-filtered Thames Water.

3 flasks incubator.	3 flasks incubator.
3 „ refrigerator.	3 „ refrigerator.

Steamed Thames Water.

3 flasks incubator.	3 flasks incubator.
3 „ refrigerator.	3 „ refrigerator.

UNINFECTED CONTROL WATERS.

2 flasks incubator.
2 „ refrigerator.

N.B.—The convention is adopted throughout the text of the Report of designating the flasks 1 *I*, 2 *I*, 3 *I*, and 1 *R*, 2 *R*, 3 *R*, according as they have been kept in the incubator or refrigerator respectively, and in this manner the individual flasks are readily identified.

In order to ascertain whether the infection of the water had communicated any large amount of organic matter to the water, some of the infected waters were submitted to a partial chemical analysis. This is a matter which has, unfortunately, been almost entirely neglected in previous observations on the vitality of pathogenic bacteria in potable waters, and it is obvious that if such investigations are to have anything but a negative value, the waters, after infection, must not differ materially in their chemical composition from that which they possess in their natural state.

These chemical analyses yielded the following results:—

Results of Analysis expressed in Parts per 100,000.

	Unfiltered Thames water* uninfected.	Unfiltered Thames water infected with typhoid.	Unfiltered Thames water infected with *B. coli*.
Ammonia (free) ……	0·013	0·013	0·015
„ (albuminoid)	0·016	0·020	0·028
Oxygen consumed ….	0·102	0·101	—
Chlorine …………	1·65	1·70	1·70

Thus the infection, especially in the case of the typhoid bacillus, had caused but very little increase in these ingredients.

The examination of the several flasks was conducted on the following principles:—

1. The unfiltered infected Thames water was examined by plate-cultivation from time to time in order to ascertain the changes in the

* The full analysis of this water is given on p. 409.

total number of micro-organisms present, but without any hope of counting or even identifying the typhoid or coli colonies, as this is, for the reasons already given (see pp. 406, 407) in general, quite out of the question.

On the other hand, the presence or absence of living typhoid and coli bacilli was periodically determined by cultivation with phenol-broth (see p. 407), a method which, of course, does not permit of an estimation of their number, but which, as will be seen, is often able to throw light on their relative abundance or on their relative degree of vitality.

2. The sterile (porcelain-filtered and steamed) infected Thames waters were periodically examined both by plate cultivation and by the phenol-broth method: so that in the case of these waters, in which the typhoid or coli bacilli were not mixed with other bacterial forms, not only could their presence or absence in the living state be determined, but their actual numbers ascertained.

3. The unfiltered uninfected Thames water was periodically examined by plate cultivation in order to follow the increase or decrease in the numbers of the water bacteria, whilst examinations by the phenol-broth method were also made in order to ascertain whether there were any forms amongst the water bacteria which might be confounded with the typhoid or coli bacilli, and thus to check the diagnoses made in the case of the unsterilised infected waters.

1. *Bacteriological Examination of the Uninfected Unsterilised Thames Water.* (First Series.)

It will be most convenient to consider first the behaviour of the control waters which were placed under the same conditions, in the incubator and refrigerator, as the infected ones.

The results of the gelatine plate cultivations of these control waters are summarised in the following table :—

Uninfected Unsterilised Thames Water (First Series). (Date of Collection of Sample at Hampton, 4.5.1892.)

Dates on which plate cultivations were made.	Particular flask employed.		Number of days plates were incubated.	Volume of water employed for plate cultivation.	Number of colonies obtained from 1 c.c. of water.	
	Incubator.	Refrigerator.			Incubator flask.	Refrigerator flask.
11.5.1893	Before subdivision.		4	c.c. $\frac{1}{6}$, $\frac{1}{12}$, $\frac{1}{50}$, and $\frac{1}{100}$	290	
16.5.1893	1 I		3	$\frac{1}{60}$ and $\frac{1}{100}$	45,000	
,,		1 R	3	$\frac{1}{60}$ and $\frac{1}{120}$		563,000
22.5.1893	1 I		3	$\frac{1}{60}$ and $\frac{1}{120}$	30,000	
,,		I R	3	$\frac{1}{50}$ and $\frac{1}{100}$		166,000
29.5.1893	1 I		2	$\frac{1}{6}$, $\frac{1}{12}$, $\frac{1}{50}$, and $\frac{1}{100}$	28,000	
,,		1 R	2	$\frac{1}{6}$, $\frac{1}{12}$, and $\frac{1}{60}$		37,(00
20.6.1893	1 I		2	$\frac{1}{6}$, $\frac{1}{12}$, $\frac{1}{60}$, and $\frac{1}{120}$	44,000	
,,		1 R	3	$\frac{1}{6}$, $\frac{1}{12}$, $\frac{1}{60}$, and $\frac{1}{120}$		9,000

The sample of Thames water which between the date of collection (4.5.1893) and the date of first examination (11.5.1893) had remained in bottles almost completely filled up to the stopper, and at a temperature of about 10—12° C. exhibited in the first instance an unusually small number of bacteria (only 290 in 1 c.c.). There can be little doubt that the original number present must have been greater than this, and have become diminished during this period of residence in the stoppered bottles, for on introduction into the flasks plugged with cotton-wool they underwent enormous multiplication. In the flask kept at 19° C. the multiplication was doubtless most rapid, but had already fallen again to 45,000 per c.c. on the second examination, whilst in the flask kept at 6° C. multiplication and subsequent decline were probably both less rapid, so that on the occasion of the second examination the number present was still 563,000 per c.c., which underwent continuous diminution during the remainder of the time that this flask was kept under observation.

These phenomena of initial multiplication followed by decline have been already frequently called attention to, both in the former Reports and by other observers, so that there is no necessity to dwell further upon it here beyond pointing out that it shows that the water-bacteria in this sample of water employed were in an active and flourishing state under the conditions maintained during the experiment.

Examination by Plate-Cultivation of the Unsterilised Thames Water infected with Typhoid and B. coli communis respectively.

Having in the previous pages traced the numerical changes which took place in the bacterial contents of the control uninfected unsterilised Thames water, I will now proceed to describe what occurred in the case of the same unsterilised Thames water which was infected with typhoid and coli respectively (in the manner indicated on p. 410).

The flasks containing these infected unsterilised Thames waters were kept at a winter (6—8° C.) and a summer (19° C.) temperature respectively, and were examined from time to time by gelatine plate cultivation, and the results of these periodical examinations are recorded in the following table:—

Unsterilised Thames Water infected with the Typhoid Bacillus on 11.5.1893.

Dates on which plate cultivations were made.	Particular flask employed.		Number of days plates were incubated.	Volume of water employed for plate cultivation.	Number of colonies obtained from 1 c.c. of water.		Remarks.
	Incubator.	Refrigerator.			Incubator flask.	Refrigerator flask.	
11.5.1893	Before subdivision		4	c.c. $\frac{1}{5}$, $\frac{1}{10}$, $\frac{1}{50}$, and $\frac{1}{100}$	78,000		On all the plates, almost all the colonies had the appearance of being possibly or probably typhoid, and inasmuch as the uninfected water on the same day gave only 290 colonies per 1 c.c., it is obvious that nearly all the colonies on these plates *must* have been typhoid.
16.5.1893	1 I		3	$\frac{1}{5}$, $\frac{1}{5}$, and $\frac{1}{100}$	31,000		There was a large number of liquefying colonies on these plates, but also a number of very small colonies which may very probably have been typhoid colonies. From the large number of liquefying colonies, it is obvious that the water bacteria must have undergone extensive multiplication. On the plates obtained from the very small volumes of water a large number of the colonies were easily recognisable as typhoid.
"		1 R	3	$\frac{1}{100}$ and $\frac{1}{500}$		342,000	
17.5.1893	1 I		3	$\frac{1}{120}$ and $\frac{1}{500}$	30,000		Many of the colonies were again easily recognisable as typhoid.
"		1 R	3	$\frac{1}{120}$ and $\frac{1}{500}$		485,000	

Unsterilised Thames Water infected with the Typhoid Bacillus on 11.5.1893—*continued.*

Dates on which plate cultivations were made.	Particular flask employed.		Number of days plates were incubated.	Volume of water employed for plate cultivation.	Number of colonies obtained from 1 c.c. of water.		Remarks.
	Incubator.	Refrigerator.			Incubator flask.	Refrigerator flask.	
				c.c.			
22.5.1893	1 I		3	$\frac{1}{50}$ and $\frac{1}{100}$	18,000		There was a large number of liquefying colonies on these plates. The recognition of typhoid colonies had become a matter of great uncertainty.
,,		1 R	3	$\frac{1}{10}$, $\frac{1}{10}$, $\frac{1}{50}$, and $\frac{1}{150}$		159,000	
29.5.1893	1 I		2	$\frac{1}{10}$, $\frac{1}{10}$, $\frac{1}{50}$, and $\frac{1}{100}$	35,000		On these plates again there was a large number of liquefying colonies, also many small colonies, but recognition of typhoid involved in complete uncertainty.
,,		1 R	2	$\frac{1}{15}$, $\frac{1}{15}$, $\frac{1}{50}$, and $\frac{1}{150}$		126,000	
20.6.1893	1 I		2	$\frac{1}{10}$, $\frac{1}{10}$, $\frac{1}{50}$, and $\frac{1}{150}$	22,000		Many liquefying colonies on these plates again, and no diagnosis of typhoid possible.
,,		1 R	2	$\frac{1}{15}$, $\frac{1}{15}$, $\frac{1}{50}$, and $\frac{1}{150}$		5,000	

From the above table it will be seen that the unsterilised Thames water, which contained remarkably few bacteria (only 290 per c.c.) at the time, was infected with a very large number of typhoid bacilli (about 78,000 per c.c.). The total number of bacteria in the water kept at the winter temperature of 6—8° C. underwent enormous multiplication followed by decline, as in the case of the uninfected unsterilised water. In the water kept at the summer temperature of 19° C., on the other hand, the numbers found exhibited almost continuous decline, and also closely resembled those found in the uninfected unsterilised water preserved under similar conditions. In both cases, however, there must have been a great multiplication of the water-bacteria, for whilst the gelatine plates prepared from these waters during the first week after infection admitted of the ready recognition of typhoid colonies, in the subsequent examinations this was altogether impossible, so that the large number of colonies present on these later plate cultivations must have been derived from the extensive multiplication of the comparatively few water-bacteria present in this unsterilised water at the outset of the experiments.

I must, however, again emphasize what I have stated before, that whilst the recognition of typhoid colonies on such plates containing the colonies of numerous water-bacteria is often difficult and attended with much uncertainty, any estimation of the number of typhoid colonies on such plates, as has been attempted by some observers, is altogether illusory and calculated to lead to the most erroneous conclusions. For whilst the surface colonies of the typhoid bacillus are even liable to be confounded with the surface colonies of some other bacteria, in the appearance of the depth colonies (and, of course, in ordinary gelatine plates the majority of the colonies are beneath the surface) there is nothing to distinguish them from an immense number of other forms common in water. Thus, whilst in the above series of examinations I have no hesitation in saying that on the plates prepared on the 11th, 16th, and 17th May, typhoid colonies were present, I rely for the determination of their presence or absence after those dates entirely on the results of the examinations by phenol broth-culture which will be given below. Again, even in the case of those plates which obviously contained typhoid colonies, I do not consider that any estimate of their number could be justifiably made, as such an estimate could only include the surface colonies which had developed the characteristic expansions.

Thus the examination by plate-culture of these unsterilised waters does not enable us to ascertain whether the typhoid bacilli underwent any numerical increase in these waters, but from the fact that no such increase was observed in the case of the typhoid bacilli similarly introduced into steam-sterilised Thames water (see p. 451), and in which, therefore, the conditions for their multiplication were

Unsterilised Thames Water infected with the *B. coli communis* on 11.5.1893.

Date on which plate cultivations were made.	Particular flask employed.		Number of days plates were incubated.	Volume of water employed for plate cultivation.	Number of colonies obtained from 1 c.c. of water.		Remarks.
	Incubator.	Refrigerator.			Incubator flask.	Refrigerator flask.	
11.5.1893 „	Before subdivision		4	c.c. $\frac{1}{50}$ and $\frac{1}{100}$	166,000		Nearly all the colonies on the plates had the typical appearance of those of *B. coli communis*.
16.5.1893 „	1 I	1 R	3 3	$\frac{1}{100}$ and $\frac{1}{250}$ $\frac{1}{100}$ and $\frac{1}{250}$	187,000	800,000	All the plates exhibited a large number of liquefying colonies, showing that the few water bacteria originally present must have undergone extensive multiplication. There were also a great many small colonies, doubtless to a large extent those of the *B. coli communis*.
23.5.1893 „	1 I	1 R	3 3	$\frac{1}{150}$ and $\frac{1}{300}$ $\frac{1}{150}$ and $\frac{1}{300}$	26,000	321,000	Again a very large number of liquefying colonies present.
30.5.1893 „	1 I	1 R	2 3	$\frac{1}{100}$ and $\frac{1}{250}$ $\frac{1}{150}$ and $\frac{1}{300}$	40,000	7,000	There were no surface colonies on these plates resembling those of the *B. coli communis*.
14.6.1893 „	2 I	1 R	2 2	$\frac{1}{150}$ and $\frac{1}{300}$ $\frac{1}{50}$ and $\frac{1}{100}$	39,000	7,500	Flask 1 I was not used, because a little of the cotton-wool stopper got into the water on the occasion of the last examination. All the plates exhibited a large number of colonies, causing liquefaction of the gelatine, and hence necessitated early counting.
21.6.1893 „	2 I	1 R	2 2	$\frac{1}{150}$ and $\frac{1}{300}$ $\frac{1}{100}$ and $\frac{1}{250}$	74,000	10,000	All the plates again exhibited a large number of colonies, causing liquefaction of the gelatine, and hence necessitated early counting.

far more favourable, there can be no reasonable doubt that they did not undergo any increase but only decline; this supposition is, moreover, corroborated by the results of the examinations by phenol broth-culture, to which I shall presently refer.

I will now turn to the similar examinations made by gelatine plate-culture of the same unsterilised Thames water infected with the *B. coli communis*, the results of which are recorded in the table on p. 418.

The results recorded in the above table for the *B. coli communis* are almost precisely parallel to those recorded in the previous table for the typhoid bacillus. There is again, in the case of the water kept at the winter temperature of 6—8° C., the enormous multiplication in the total number of bacteria present, followed by rapid and almost continuous subsequent decline. In the case of the water kept at the summer temperature of 19° C., a slight increase was observed on the occasion of the second examination (but, as pointed out in the case of the typhoid table, a great increase followed by rapid decline may have taken place in the interval between the first and second examinations), after which there was a great decline followed by some recrudescence at the end. In the case of the waters kept both at the winter and the summer temperatures respectively, however, it is obvious that extensive multiplication of the water-bacteria must have taken place, owing to the very large increase in the number of colonies causing liquefaction of the gelatine which was observed.

For the same reasons as stated in the case of the typhoid bacillus (see p. 417), it is impossible to form any estimate of the numbers in which the coli bacilli were present after the day (11.5.1893) of their introduction, nor as to the length of time over which they persisted in the living state in these waters. From the corresponding experiments, however, made with the steam-sterilised Thames water, it is quite possible that the *B. coli communis*, unlike the typhoid bacillus, may have undergone some multiplication in the water. It is to the examinations by the method of phenol broth-culture that we must again have recourse in order to ascertain how long the coli bacilli remained alive in these unsterilised waters.

There is a point which is brought out very strikingly in these tables, and to which I would draw attention at this stage, and that is that the total number of bacteria present in these unsterilised waters at the end of the period over which these experiments extended, was, both in the case of the uninfected waters (see table, p. 413) as well as in that of the typhoid (see table, p. 415), and in that of the coli (see table, p. 418), greater in the water maintained at the summer than at the winter temperature respectively. The probable explanation of this phenomenon would appear to be that at the lower temperature (6—8° C.) many of the bacteria present may be unable to form spores,

Vitality of the Typhoid Bacillus in Various Waters.

Investigators and Date of Experiments.	Source of Organism.	Temperature at which water was maintained.	Foul Water.	Ordinary Potable Water Unsterilised.	Ordinary Potable Water Sterilised.	Distilled Water.	Mineral Waters.	Sea-water or Concentrated Salt Solution.	Remarks.
Braem[1] (1889) ...	—	—	—	—	—	188 days.	—	—	*Sterilised distilled water.
Freytag[2] (1890) ...	—	—	—	—	—	—	—	5 months	Concentrated salt solution.
Giaxa[3] (1889) ...	Two days old agar-agar culture grown at 36° C.; 1 needle-point taken.	—	—	—	—	—	—	Still present on the 9th day.†	†Unsterilised sea-water.
	Two drops broth-culture 8 days old and kept at 36° C.	—	—	—	—	—	—	Still present in large numbers on the 25th day.‡	‡Sterilised sea-water.
Heraeus[4] (1886) ...	Small quantity taken from a 'streaked culture.'	37° C.	Multiplied from 2 millions to 160 millions in 2 days.*						*Unfiltered River Spree water sterilised.
		12° C.	Multiplied from 12,000 to 87,000 in 2 days.						

Hochstetter[5] (1887).	Potato-cultures grown for 4 and 7 days at temperature of room and at 36° C. Portions of the growth were mixed with sterilised distilled water and inoculated into the various waters.*	12°–15° C.	—	—	Longest duration of vitality observed, 7 days.†	Longest duration of vitality observed, 5 days.§	Longest duration of vitality observed, 5 days.¶	—	*Hochstetter states that he could detect no difference in the behaviour of the cultures grown at different temperatures. †Sterilised Berlin tap-water. §Sterilised distilled water. ¶Seltzer-water.
Hueppe[6] (1887) ...	— Taken from potato-cultures 5 days old.	10°–20° C. 15°–20° C. 10° C.	— Over 30 days: none found, however, on the 60th day.† Rapid diminution in 2 out of 5 experiments none were found on the 10th day.†	—	20 to 30 days.*	—	—	—	*Sterilised Wiesbaden tap water. †Unsterilised polluted well-water. †Ditto.
Karlinski[7] (1889)...	—	8° C.	—	6 days.*	—	—	—	—	*Unsterilised Innsbruck drinking water.
Kraus[8] (1887) ...	— —	10·5° C. 10·5° C.	— —	5 to 7 days, no longer demonstrable on 7th day.* No longer demonstrable on 7th day.†	—	—	—	—	*Unsterilised well-water. †Unsterilised Munich Mangfall water. Considered a very pure water.
Maschek[9] (1887) ...	—	18°–22° C.	—	—	10 to 30 days.*	—	—	—	Leitmeritz town water sterilised.
Mattei and Stagnitta[10] (1889)	—	8°–12° C.	—	—	4 days.				

Vitality of the Typhoid Bacillus in Various Waters—*continued.*

Investigators and Date of Experiments.	Source of Organism.	Temperature at which water was maintained.	Foul Water.	Ordinary Potable Water Unsterilised.	Ordinary Potable Water Sterilised.	Distilled Water.	Mineral Waters.	Sea-water or Concentrated Salt Solution.	Remarks.
Meade Bolton[11] (1886).	Small quantities taken from either sloped agar-agar or gelatine-cultures and mixed with sterilised ordinary salt solution from which a few drops were taken and mixed with 10 c.c. of the water under investigation.	20° C.	Over 40 days.†	—	Over 7 days.§	From 2–3 and 10–14 days. None were found between 30 and 40 days.*	—	—	*Sterilised distilled water. †Highly polluted well-water sterilised. §Ordinary Göttingen water supply, and containing very little organic matter. Sterilised.
		35° C.	From 10–14 days. None found after 20–24 or 30–40 days.†	—	—	from 2–3 days. None found after 6-7 or 10-14 or 20–24 days.*			
Pfeiffer[12] (1886) ...	—	—	—	—	Upwards of 4 months.*	—	—	—	*Sterilised well-water.
Straus and Dubarry[13] (1889).	One needle-point of a potato-culture of the bacillus introduced into 10 c.c. of the water under examination.	20° C.	—	—	32 days.* 43 days.†	30-35 days.§	—	—	*Sterilised Ourcq water. †Sterilised Vanne water. The latter has less organic matter than the Ourcq water.
		25° C. 35° C.	— —	— —	81 days.* 37 days.*	69 days.§ 27 days.§			§Sterilised distilled water.
Uffelmann[14] (1888)	—	Ordinary temperature of a room.	—	2 weeks.*	—	—	—	—	*Well water in Rostock, unsterilised.

Wolffhügel and Riedel[15] (1886).	One needle-point from a gelatine culture introduced into 50 c.c. of the water.	18°-22° C.	—	—	Over 32 days.*	—	—	—	*Sterilised Berlin tap water. †Sterilised distilled water. §Sterilised highly polluted River Panke water.
	One needle-loop from a broth-culture.	15°-20° C.	—	—	—	Over 15 days.†			
	One needle-point from a gelatine-culture introduced into 10 c.c. of the water.	35° C.	Over 10 days.§						
Slater[16] (1893) ...	Culture on agar, 37° C., 24 and also 48 hours old, inoculated into sterile distilled or sterile soda-water, 1·5 to 2 c.c. of which were employed for each inoculation.	Ordinary temperature.	—	—	—	Alive 50 days after inoculation.†	11 days, not found on the 13th day.* Dead on 5th day.‡ 8 days, dead on the 9th day.§	—	†Sterile distilled water. *Simple aërated non-sterile. ‡Sterile soda-water. §Sterile soda-water, non-aërated.

1 "Untersuchungen über die Degenerationserscheinungen pathogener Bakterien im destillirten Wasser," 'Beiträge zur pathologischen Anatomie und zur allgemeinen Pathologie,' vol. 7, p. 11.
2 "Ueber die Einwirkung concentrirter Kochsalzlösungen auf das Leben von Bakterien," 'Archiv für Hygiene,' vol. 11, 1890, p. 60.
3 "Ueber das Verhalten einiger pathogener Mikroorganismen im Meerwasser," 'Zeitschrift für Hygiene,' vol. 6, 1889, p. 162.
4 "Ueber das Verhalten der Bacterien im Brunnenwasser," 'Zeitschrift für Hygiene,' vol. 1, 1886, p. 193.
5 "Ueber Mikroorganismen im künstlichen Selterwasser," 'Arbeiten aus dem Kaiserlichen Gesundheitsamte,' vol. 2, 1887, p. 1.
6 "Die hygienische Beurtheilung des Trinkwassers vom biologischen Standpunkte," 'Schilling's Journal für Gasbeleuchtung und Wasserversorgung,' 1887. Separat-Abdrück, p. 130.
7 "Ueber das Verhalten einiger pathogener Bakterien im Trinkwasser," 'Archiv für Hygiene,' vol. 9, 1889, p. 113.
8 "Ueber das Verhalten pathogener Bakterien im Trinkwasser," 'Archiv für Hygiene,' vol 6, 1887, p. 234.
9 "Bakteriologische Untersuchungen der Leitmeritzer Trinkwässer," 'Jahresbericht der Oberrealschule zu Leitmeritz,' 1887.
10 "Sur la manière d'être des microbes pathogènes dans l'eau courante," 'Annali dell Istituto d'Igiene speriment. di Roma,' 1889.
11 "Ueber das Verhalten verschiedener Bakterienarten im Trinkwasser," 'Zeitschrift für Hygiene, vol. 1, 1886, p. 76.
12 "Die Beziehung der Bodencapillarität zum Transport von Bacterien," 'Zeitschrift für Hygiene,' vol. 1, 1886, p. 398.
13 "Recherches sur la durée de la vie des microbes pathogènes dans l'eau," 'Archives de Médecine expérimentale et d'Anatomie pathologique,' vol. 1, 1889, p. 5.
14 "Trinkwasser und Infectionskrankheiten," 'Wiener medicinische Presse,' 1888, No. 37 ('Centralblatt für Bakteriologie,' vol. 5, 1889, p. 89).
15 "Die Vermehrung der Bacterien im Wasser," 'Arbeiten aus dem Kaiserlichen Gesundheitsamte,' vol. 1, 1886, p. 455.
16 "Investigation of Artificial Mineral Waters," 'Journal of Pathology and Bacteriology,' vol. 1, 1893, p. 468.

and thus perish by the long residence in the water, whilst at the higher temperature (19° C.), although the fully developed bacteria are more rapidly destroyed, a larger proportion of them give rise to spores and thus lead to a larger permanent bacterial population in the water.

Examination of the Unsterilised Thames Waters, Infected and Uninfected by Phenol Broth-culture.

I must now call attention to the results of the phenol broth-cultivations made both with the unsterilised Thames water, as well as with that infected with the typhoid bacillus and the *B. coli communis* respectively.

The following experiment will show how under favourable conditions, the phenol broth test serves to distinguish a water containing typhoid bacilli from another in which they are absent; thus

Phenol Broth Experiments (12.5.1893).

(1) 1 c.c. uninfected unsterilised Thames water added to 10 c.c. broth + 5 drops phenol solution.

(2) 1 c.c. uninfected unsterilised Thames water added to 10 c.c. broth + 3 drops phenol solution.

(3) 0·5 c.c. uninfected unsterilised Thames water added to 10 c.c. broth + 5 drops phenol solution.

(4) 0·5 c.c. uninfected unsterilised Thames water added to 10 c.c. broth + 3 drops phenol solution.

(5) 1 c.c. unsterilised Thames water infected with typhoid added to 10 c.c. broth + 5 drops phenol solution.

(6) 1 c.c. unsterilised Thames water infected with typhoid added to 10 c.c. broth + 3 drops phenol solution.

(7) 0·5 c.c. unsterilised Thames water infected with typhoid added to 10 c.c. broth + 5 drops phenol solution.

(8) 0·5 c.c. unsterilised Thames water infected with typhoid added to 10 c.c. broth + 3 drops phenol solution.

These eight tubes, all in duplicate, were placed in an incubator at 38° C., and on the following day, whilst all the uninfected tubes, 1, 2, 3, and 4, were clear, all the infected tubes, 5, 6, 7, and 8, were turbid, thus showing that whilst the addition of the phenol solution had prevented the proliferation of the ordinary water-bacteria in the uninfected Thames water, the extensive multiplication of the typhoid bacilli in the infected Thames water had taken place in spite of the presence of the same proportions of phenol. In the same way, on the following day, similar quantities of the unsterilised Thames water infected with the *B. coli communis* were introduced into broth-

tubes, to which the same proportions, as above, of phenol solution were added, and all these tubes similarly became turbid on being kept at 38° C. for twenty-four hours.

Thus at the outset of this series of experiments, the uninfected Thames water was sharply distinguishable by means of the phenol broth test from the same Thames water after infection with either the typhoid bacillus or the *B. coli communis*.

The uninfected and infected unsterilised Thames waters were again compared by the method of phenol broth cultivation on 29.5.1893.

Number of broth tube.	Water used.	Quantity of water taken. c.c.	Quantity of phenol solution added to 10 c.c. broth.	Remarks.
	Unsterilised Uninfected Thames.			
(1)	Flask 1 I ..	0·5	5 drops	No turbidity even on 6.6.1893.
(2)	" ..	1·0	"	
(3)	Flask 1 R..	0·5	"	
(4)	" ..	1·0	"	
	Unsterilised Typoid-infected Thames.			
(5)	Flask 1 I ..	0·5	"	Did not become turbid.
(6)	" ..	1·0	"	Turbid in 48 hours.
(7)	Flask 1 R..	0·5	"	Turbid in 24 hours.
(8)	" ..	1·0	"	" "

The results recorded in the above table indicated that on 29.5.1893, whilst there were still no bacteria in the uninfected unsterilised Thames water to interfere with the phenol broth test, this test pointed to the presence of living typhoid bacilli in the typhoid-infected Thames waters, which had been kept both at 6° C. and at 19° C. (flasks 1 R and 1 I). The results of the test, moreover, indicate that these typhoid bacilli were now less numerous or in a less active condition in the flask 1 I (19° C.) than in the flask 1 R (6° C.), because both phenol broth tubes prepared from 1 R became turbid already in twenty-four hours, whilst of the two similar tubes prepared from flask 1 I, only the one in which 1 c.c. of water was employed for cultivation

became turbid, and then only after forty-eight hours, whilst the tube in which only 0·5 c.c. of water was employed did not become turbid at all.

It must not, however, be supposed that the diagnosis of typhoid bacilli in these waters was allowed to rest on such slender evidence as the mere clouding of these phenol broth-cultures, but the latter were submitted to gelatine plate cultivation to see if the characteristic typhoid colonies made their appearance, and these colonies were further confirmed by inoculation (*a*) on to potatoes for exhibition of the characteristic growth, (*b*) into gelatine tubes to see if bubbles of gas would make their appearance, (*c*) into broth for the indol test, and generally also (*d*) into milk to see whether coagulation of the casein would take place. Thus in the case of the above phenol broth-cultures commenced on 29.5.1893, the final confirmation of typhoid was not obtained until 12.6.1893, or a fortnight later.

The phenol broth test was again applied to the waters on 5.6.1893, with the following results (p. 427).

The plate cultivations made from the phenol broth tubes, referred to in the table (p. 427), yielded the following results :—

Broth tube.

(21.) *Typhoid-infected Unsterilised Thames, Flask* 1 I. (*Typhoid Present.*)

The presence of typhoid was confirmed by the typical appearance of colonies, growth on potatoes, negative indol test, and negative gelatine bubble test.

(23.) *Typhoid-infected Unsterilised Thames, Flask* 1 R. (*Typhoid Present.*)

The presence of typhoid was confirmed by the typical colonies, growth on potatoes, negative indol, and negative gelatine bubble tests.

(37.) and (45.) *Coli-infected Unsterilised Thames, Flask* 2 I. (*B. coli Present.*)

The presence of the *B. coli* was confirmed by typical colonies, growth on potatoes, positive indol, and positive gelatine bubble tests.

(39.) and (47.) *Coli-infected Unsterilised Thames, Flask* 1 R. (*B. coli Present.*)

The presence of the *B. coli* was confirmed by typical colonies, growth on potatoes, positive indol, and positive gelatine bubble tests.

Examination by Phenol Broth-culture on 5.6.1893.

Number of broth tube.	Water used for cultivation with phenol broth.	Quantity of water taken in c.c.	Quantity of phenol solution added to 10 c.c. broth.	Remarks.
		Unsterilised Typhoid-infected Thames.		
(21)	Flask 1 I ..	0·5	3 drops	Turbid in 48 hours. Plates poured 9.6.1893.
(22)	" ..	1·0	"	Turbid in 48 hours.
(23)	Flask 1 R..	0·5	"	Turbid in 24 hours. Plates poured 6.6.1893.
(24)	" ..	1·0	"	Turbid in 24 hours.
		Unsterilised Coli-infected Thames.		
(37)	Flask 2 I ..	0·5	3 drops	Turbid in 24 hours. Plates poured 6.6.1893.
(38)	" ..	1·0	"	Turbid in 24 hours.
(39)	Flask 1 R..	0·5	"	Turbid in 24 hours. Plates poured 6.6.1893.
(40)	" ..	1·0	"	Turbid in 24 hours.
		Unsterilised Uninfected Thames.		
(41)	Flask 1 I ..	0·5	3 drops	Turbid in 48 hours. Plates poured 9.6.1893.
(42)	" ..	1·0	"	Turbid in 48 hours.
(43)	Flask 1 R ..	0·5	"	Turbid in 48 hours. Plates poured 9.6.1893.
(44)	" ..	1·0	"	Turbid in 48 hours.
		Unsterilised Coli-infected Thames.		
(45)	Flask 2 I ..	0·5	5 drops	Turbid in 24 hours. Plates poured 6.6.1893.
(46)	" ..	1·0	"	Turbid in 24 hours.
(47)	Flask 1 R..	0·5	"	Turbid in 24 hours. Plates poured 6.6.1893.
(48)	" ..	1·0	"	Turbid in 24 hours.

Broth tube.

(41.) *Uninfected Unsterilised Thames, Flask* 1 I.

The only colonies on the plate which bore any resemblance to typhoid, yielded pink growths on potatoes, and developed green fluorescence on being grown in gelatine tubes; they were thus in reality wholly unlike and different from typhoid or coli.

Broth tube.

(43.) *Uninfected Unsterilised Thames, Flask* 1 R.

Colonies bearing some resemblance to typhoid yielded growths on potatoes, which were also not unlike typhoid, but on inoculation into gelatine tubes a green fluorescence was obtained, conclusively proving that it was another organism and not typhoid or coli.

From these examinations then it was apparent that on this day, 5.6.1893,—

1. *The unfiltered Thames water infected with* typhoid *on* 11.5.1893, *or twenty-five days previously, still contained living typhoid bacilli, both in that portion of the water which had been preserved at* 19° *C. as well as in that kept at* 6° *C., the number and vital activity of the typhoid bacilli being apparently greater in the latter than in the former.*

2. *The unfiltered Thames water infected with the* B. coli communis *on the same date also still contained these bacilli in a living state, both in that portion of the water kept at* 19° *C. as well as in that maintained at* 6° *C.*

3. *The unfiltered uninfected Thames water which had been maintained under exactly similar conditions, contained no bacteria which after careful examination could be mistaken either for the typhoid bacillus or the* B. coli communis.

The unfiltered infected waters were again examined on 14.6.1893, with the following results:—

Examination by Phenol Broth-culture on 14.6.1893.

Number of broth tube.	Water used for cultivation with phenol broth.	Quantity of water taken in c.c.	Quantity of phenol solution added to 10 c.c. broth.	Remarks.
				Unsterilised Typhoid-infected Thames.
(85)	Flask 1 I ..	1·0	3 drops	Turbid in 48 hours. Plates poured.
(86)	Flask 1 R ..	1·0	„	„ „
				Unsterilised Coli-infected Thames.
(87)	Flask 1 I ..	1·0	3 drops	Turbid in 24 hours. Plates poured.
(88)	Flask 1 R ..	1·0	„	„ „

The plate cultivations made from the above phenol broth tubes gave the following results:—

Broth tube.

(85.) *Unsterilised Typhoid-infected Thames, Flask* 1 I. (*Typhoid Absent.*)

The plate exhibited many liquefying colonies as well as a large number of very small colonies; the latter were placed on potatoes, and yielded light brown growths; on inoculation into gelatine tubes liquefaction took place, therefore certainly not typhoid.

(86.) *Unsterilised Typhoid-infected Thames, Flask* 1 R. (*Typhoid Absent.*)

The small colonies on the plate which alone exhibited *any* resemblance to typhoid were examined as in No. 85 above, and yielded exactly similar results, therefore certainly not typhoid.

(87.) *Unsterilised Coli-infected Thames, Flask* 1 *Incubator.* (*B. coli Present.*)

Almost pure cultivation, with numerous typical extension colonies like *B. coli;* these yielded characteristic growth on potatoes, gave the indol reaction, and the gas-bubbles in gelatine tube. *B. coli*, therefore, present.

(88.) *Unsterilised Coli-infected Thames, Flask* 1 *Refrigerator.* (*B. coli Present.*)

Exactly similar results with this as with No. 87 above. *B. coli*, therefore, present.

From these examinations it appears that on 14.6.1893,—

1. *The* typhoid *bacillus was no longer demonstrable in the unsterilised Thames water, which had been infected with it thirty-four days previously. It had disappeared both in that portion of the water which had been preserved at a winter, as well as in that kept at a summer, temperature.*

2. *The* B. coli communis, *on the other hand, was easily demonstrable in similar water which had been preserved under precisely the same conditions.*

These infected unsterilised Thames waters were again submitted to examination on 21.6.1893, with the following results:—

Examination by Phenol Broth-culture on 21.6.1893.

Number of broth tube.	Water used for cultivation with phenol broth.	Quantity of water taken in c.c.	Quantity of phenol solution added to 10 c.c. broth.	Remarks.
		Typhoid-infected Unsterilised Thames.		
(86A)	Flask 1 I ..	1·0	3 drops	Turbid in 48 hours. Plates poured 28.6.1893.
(87A)	Flask 1 R..	1·0	"	Not turbid on 4.7.1893.
		Coli-infected Unsterilised Thames.		
(94)	Flask 2 I ..	1·0	3 drops	Turbid in 24 hours. Plates poured 22.6.1893.
(95)	Flask 1 R..	1·0	"	" " " "

The plate cultivations made from the above phenol broth tubes gave the following results:—

Broth tube.

(86A.) *Typhoid-infected Unsterilised Thames, Flask* 1 *Incubator. (Typhoid Absent.)*

The colonies were inoculated on to potatoes, on which a light brown growth extending over the whole potato was obtained. On inoculation into gelatine tubes liquefaction followed, therefore certainly not typhoid. No bubbles of gas appeared in the gelatine.

(94.) *Coli-infected Unsterilised Thames, Flask* 2 *Incubator. (B. coli Present.)*

The plate had the appearance of being a pure cultivation of *B. coli*, with the characteristic colonies, yielding characteristic growth on potatoes, also indol reaction, and gas bubbles on inoculation into melted gelatine tubes. *B. coli*, therefore, present.

(95.) *Coli-infected Unsterilised Thames, Flask* 1 *Refrigerator. (B. coli Present.)*

Exactly similar results were obtained with this as with No. 94 above. *B. coli*, therefore, present.

Thus, on this day (21.6.1893) *as on the occasion of the previous examination* (14.6.1893), *the typhoid bacilli were no longer demonstrable in the unsterilised Thames water into which they had been introduced on* 11.5.1893. *The* B. coli communis, *on the other hand, was again easily discoverable under the same circumstances,* i.e., *forty days after its introduction into the unsterilised Thames water.*

The typhoid-infected unsterilised Thames water was again similarly examined on 26.6.1893, with the following results :—

Examination by Phenol Broth-culture on 26.6.1893.

Number of broth tube.	Water used for cultivation with phenol broth.	Quantity of water taken in c.c.	Quantity of phenol solution added to 10 c.c. broth.	Remarks.
		Typhoid-infected Unsterilised Thames.		
(96)	Flask 1 I ..	1·0	3 drops	Turbid in 48 hours. Plates poured.
(97)	Flask 1 R ..	1·0	"	" " " "

The plate cultivations made from the above phenol broth tubes yielded the following results :—

Broth tube.

(96.) *Typhoid-infected Unsterilised Thames, Flask* 1 *Incubator.* (*Typhoid Absent.*)

The plates contained some small colonies presenting some resemblance to typhoid ; on being transferred to potatoes they yielded light brown growths unlike typhoid, and on being inoculated into gelatine green fluorescence without liquefaction was obtained. These colonies were, therefore, not those of typhoid.

(97.) *Typhoid-infected Unsterilised Thames, Flask* 1 *Refrigerator.* (*Typhoid Absent.*)

The plates exhibited two types of colony, firstly, liquefying ones which could not be typhoid, and, secondly, small depth colonies, which on transference to potatoes gave thick greyish-brown growths wrinkled in parts, and on inoculation into gelatine tubes caused subsequent liquefaction. These were, therefore, not typhoid.

This examination, made on 26.6.1893, *confirmed, therefore, the two previous examinations of* 21.6.1893 *and* 14.6.1893, *which showed that the typhoid bacillus, introduced on* 11.5.1893, *was no longer discoverable in the unsterilised Thames water.*

The final examination of these waters was made on 5.7.1893, thus—

Examination by Phenol Broth-culture on 5.7.1893.

Number of broth tube.	Water used for cultivation with phenol broth.	Quantity of water taken in c.c.	Quantity of phenol solution added to 10 c.c. broth.	Remarks.
		Unsterilised Uninfected Thames.		
(106)	Flask 1 I ..	1·0	3 drops	Turbid in 48 hours, Plates poured 7.7.1893.
(107)	" 1 R.	1·0	"	Ditto.
(119)	Flask 1 I ..	1·0	5 drops	Not turbid in 6 days.
(120)	" 1 R..	1·0	"	Ditto.
		Typhoid infected Unsterilised Thames.		
(108)	Flask 1 I ..	1·0	3 drops	Turbid in 72 hours. Plates poured 8.7.1893.
(109)	" 1 R..	1·0	"	Turbid in 48 hours. Plates poured 7.7.1893.
(121)	Flask 1 I ..	1·0	5 drops	Not turbid in 6 days.
(122)	" 1 R..	1·0	"	Ditto.

Thus, whilst the broth tubes containing only 3 drops of phenol still became turbid when inoculated with both uninfected and infected waters, the broth tubes to which 5 drops of the phenol solution were added remained clear after inoculation with both waters.

The plate cultivations made from the above broth-tubes, which became turbid, yielded the following results :—

Broth tube.
(106.) *Unsterilised Uninfected Thames, Flask* 1 *Incubator.*

The plates exhibited a large number of highly fluorescent expansion colonies without liquefaction; some of the least fluorescent of these colonies, and bearing therefore a faint resemblance to typhoid, were transferred to potatoes, on which they gave rise to light brown, sharply demarcated growths, quite unlike typhoid. No gas bubbles were produced on inoculation into melted gelatine tubes.

Broth tube.

(107.) *Unsterilised Uninfected Thames Flask 1 Refrigerator.*

The plates exhibited an apparently pure cultivation of a liquefying organism, the colonies having a strong resemblance to those of *B. liquidus* (see 2nd Report, p. 186). Some of the colonies which had not yet caused liquefaction were transferred to potatoes, on which they gave rise to thick, whitish, waxy growths, quite unlike those of typhoid.

(108.) *Typhoid-infected Unsterilised Thames, Flask 1 Incubator.* *(Typhoid Absent.)*

The plates contained fluorescent expansion, liquefying and small dot colonies. All three types of colony were transferred to potatoes, on which the small dot and fluorescent expansion colonies gave rise to light brown sharply marked growths, and the liquefying colonies to shining slimy colourless growths; none of these were, therefore, similar to those of typhoid.

(109). *Typhoid-infected Unsterilised Thames, Flask 1 Refrigerator.* *(Typhoid Absent.)*

The plates appeared to be pure cultivations; the depth colonies were small dots, often oval in shape, and the surface colonies resembled very small "milk-drops;" even on the plate in which the colonies were few and far between there were none of the typical expansion colonies resembling typhoid. On transference to potatoes, shining slimy growths, having the appearance of drops of water, were obtained.

This final examination of these unsterilised typhoid-infected Thames waters therefore again confirmed the previous results, which may be thus summarised:—

(1.) The unsterilised uninfected Thames water, collected at Hampton on 5.5.1893, contained throughout the entire course of the series of experiments no bacteria resembling either the typhoid bacillus or the *B. coli communis*.

(2.) The unsterilised Thames water infected with typhoid on 11.5.1893, was still found to contain living typhoid bacilli on 5.6.1893, or twenty-five days after infection, whilst on 14.6.1893, thirty-four days after infection, they were no longer demonstrable.

(3.) These remarks apply equally to the waters preserved at a winter and a summer temperature respectively,

although there is some evidence that on the last day of their detection the typhoid bacilli were present, either in larger numbers or in a more active state in the water maintained at the winter than in that at the summer temperature.

(4.) The same unsterilised Thames water infected with the *B. coli communis* on the same day, and kept under precisely similar conditions of temperature, &c., was still found to contain living coli bacilli forty days after their introduction. These bacilli doubtless persisted in the living state, even for a much longer period of time than this, no later examinations being made; and on the occasion of their last detection they did not appear to have lost any of their original vitality, as they still promptly reacted with the phenol broth test.

Experiments on the Influence of the Addition of Salt to the Unsterilised Typhoid-infected Thames Water.

In the recent cholera epidemic at Hamburg, it is now almost universally recognised that the most important agent in distributing the zymotic poison was the highly polluted and unfiltered water of the River Elbe. This water, moreover, during the epidemic was found to be unusually rich in salt, in fact at times it was distinctly brackish in character. Thus a sample of Hamburg water sent to me by Mr. Ernest Hart in October, 1892, and which I submitted to analysis, had the following composition:—

Sample of Hamburg Water received from Mr. Ernest Hart.

Results of Analysis expressed in Parts per 100,000.

Total solid matters.	Organic carbon.	Organic nitrogen.	Ammonia. Free.	Ammonia. Albuminoid.	Nitrogen as nitrates and nitrites.	Chlorine.	Hardness. Temporary.	Hardness. Permanent.	Hardness. Total.
78·90	0·926	0·088	0·030	0·047	0	31·3	4·1	13·7	17·8

Oxygen consumed by organic matter, as measured by reduction of a solution of permanganate acting for three hours in the cold = 0·366.

The water was very turbid, depositing a quantity of brown suspended matter.

The high percentage of salt is due to the waste liquors which are discharged from the Stassfurth salt works and other factories into the Elbe and its tributaries. Now it has been more recently shown (Trenkmann, "Beitrag zur Biologie des Kommabacillus," 'Centralbl

f. Bakteriologie,' vol. 13 (1893), p. 313) that the addition of sodium chloride, and of other salts in certain proportions, to water containing the cholera bacilli causes a most remarkable multiplication of the latter, as may be seen from the following table, given by Trenkmann, which exhibits the effect of making such additions to a sterilised well water, which had been purposely infected with cholera bacilli.

Addition of various Salts to Sterilised Well Water containing Cholera Bacilli.

The waters were maintained at 21—24° C.	(Trenkmann.) Number of cholera bacilli in 1 needle loop.	
	After 24 hours.	After 8 days.
(1.) 10 c.c. sterile well water	580 520	5
(2.) " + 1 drop* 10 per cent. sodium chloride	6,120	12,480
(3.) " + 2 drops " " "	9,240	19,560
(4.) " + 3 " " " "	15,000	10,440
(5.) " + 1 drop " " nitrite	1,740	10,920
(6.) " + 2 drops " " "	6,600	1,460
(7.) " + 3 " " " "	17,160	2,260
(8.) " + 1 drop " " nitrate	8,040	4,040
(9.) " + 2 drops " " "	6,660	14,760
(10.) " + 3 " " " "	20,940	16,080
(11.) " + 1 drop " disodium phosphate	3,360	—
(12.) " + 2 drops " " "	7,560	—
(13.) " + 3 " " " "	6,540	—
(14.) " + 1 drop " sodium carbonate	7,440	—
(15.) " + 2 drops " " "	28,680	—
(16.) " + 3 " " " "	31,560	—
(17.) " + 2 " " " chloride and 1 drop 10 per cent. disodium phosphate	54,720	—

* 25—27 drops = 1 c.c.

These results show that the presence of an unusually high proportion of salts may not improbably have played an important part in the distribution of cholera by means of the water of the Elbe in the recent Hamburg epidemic, and possibly also by means of the Thames water in some of the former London epidemics, as at that time the metropolitan supply was in part derived from the tidal portion of the river.

In view of these circumstances, it appeared to me to be of considerable interest to ascertain whether and in what way the behaviour of the typhoid bacillus in water is affected by additions of salt, and, with this object, the following experiments were undertaken :—

Preparation of the Saline Waters.—The saline waters employed were of three different strengths—

(*a.*)	Containing	0·1	per cent.	sodium chloride.
(*b.*)	"	1·0	"	"
(*c.*)	"	3·0	"	"

Three portions of pure sodium chloride, weighing respectively 0·5, 5·0, and 15·0 grams, were sterilised in the air-oven at 150° C. for several hours, and then placed in three sterile 500 c.c. measuring flasks. To each of these was added sufficient of the typhoid-infected unsterilised Thames water (p. 410) to dissolve the salt, and the solution was in each case further diluted to the 500 c.c. mark with the same infected unsterilised Thames water. These saline waters were then distributed in smaller flasks plugged with cotton-wool, and two of the latter of each particular strength were placed in the refrigerator at 6—8° C., and two in the incubator at 19° C. Thus there were

Unsterilised typhoid-infected Thames water +0·1 per cent. salt	2 flasks refrigerator 2 " incubator
Unsterilised typhoid-infected Thames water +1·0 per cent. salt	2 flasks refrigerator 2 " incubator
Unsterilised typhoid-infected Thames water +3·0 per cent. salt	2 flasks refrigerator 2 " incubator

These saline waters, like the others, were prepared on 11.5.1893, and were examined by gelatine plate cultivation at frequent intervals subsequently. The results obtained must obviously be compared with those from the typhoid-infected unsterilised Thames water, to which no salt was added, and which have already been recorded in the Table on pp. 415 and 416, but which are again given, so as to facilitate comparison, in the first column of the following table :—

Typhoid-infected Unsterilised Thames Water, with and without the addition of Common Salt. Experiments commenced 11.5.1893.

Dates on which plate cultivations were made.	Number of colonies obtained from 1 c.c.							
	Water without additions.		Water + 0·1 per cent. NaCl.		Water + 1·0 per cent. NaCl.		Water + 3·0 per cent. NaCl.	
	Incubator flask.	Refrigerator flask.	Incubator flask.	Refrigerator flask.	Incubator flask.	Refrigerator flask.	Incubator flask.	Refrigerator flask.
11.5.1893	78,000 (4 days)*	78,000	(78,000)	(78,000)	(78,000)	(78,000)	(78,000)	(78,000)
17.5.1893	30,000 (3 days)	485,000 (3 days)	189,000 (3 days)	381,000 (3 days)	366,000 (3 days)	436,000 (3 days)	3,900,000 (3 days)	300 (8 days)
22.5.1893	18,000 (3 days)	159,000 (3 days)	33,000 (3 days)	262,000 (3 days)	56,000 (3 days)	1,800,000 (3 days)	3,000,000 (4 days)	194,000 (5 days)
29.5.1893	35,000 (2 days)	126,000 (2 days)	71,000 (2 days)	143,000 (2 days)	53,000 (2 days)	1,500,000 (2 days)	5,000,000 (4 days)	2,000,000 (4 days)
20.6.1893	22,000 (2 days)	5,000 (2 days)	43,000 (2 days)	9,000 (2 days)	31,000 (2 days)	110,000 (2 days)	3,600,000 (3 days)	1,700,000 (2 days)

* This refers to the number of days that the gelatine plates were incubated at 18—20° C.

From the above table it will be seen that the addition of common salt to the typhoid-infected unsterilised Thames water occasioned an enormous increase in the number of water-bacteria, the effect being most pronounced in the case of the water to which 3 per cent. addition had been made, whilst the water which had received an addition of only 0·1 per cent. of sodium chloride gave results which did not differ materially from those yielded by the water to which no salt had been added.

The addition of salt was by no means conducive to the welfare of all the different kinds of water-bacteria present, but, on the contrary, from the appearance of the plate cultivations, it was evident that some forms were favoured at the expense of others; thus, on the plates prepared from the 3 per cent. salt water, there was a conspicuous absence of liquefying colonies, these plates having, in fact, almost the appearance of pure cultivations. This entirely bears out what I found a number of years ago ("On the Multiplication of Micro-organisms," 'Proc. Roy. Soc.,' 1886), that in a water containing only a very limited number of species there is generally far more extensive multiplication than in the case of one containing many different species.

The manner in which the salt operated is most apparent from that flask of the water to which an addition of 3 per cent. had been made, and which was preserved in the refrigerator (6—8° C.), as in this case the multiplication took place most slowly. Thus, whilst the water at the outset contained 78,000 bacteria per c.c., of which nearly all were typhoid bacilli, after six days there were only 300 bacteria per c.c., so that there must have been an enormous destruction in the interval, but the bacteria left subsequently underwent enormous multiplication, although more slowly than in the case of the corresponding flask kept in the incubator (19° C.).

In all cases it will be seen that the multiplication was followed by decline, but this decline was greatly retarded in those waters which had received the large additions of salt.

We must now consider the effect of these additions of salt on the typhoid bacilli in the waters, as indicated by the results of phenol broth-culture.

These saline waters were first submitted to phenol broth-culture on 29.5.1893, with the following results:—

Examination by Phenol Broth-culture, 29.5.1893.

Number of broth tube.	Water used for cultivation with phenol broth.	Quantity of water taken in c.c.	Quantity of phenol solution added to 10 c.c. broth.	Remarks.
		Typhoid-infected Unsterilised Thames + 0·1 *per cent.* NaCl.		
(9)	Flask 1 I ..	0·5	5 drops	Did not become turbid even in seven days.
(10)	" ..	1·0	"	" " "
(11)	Flask 1 R ..	0·5	"	Turbid in 24 hours. Plates poured 30.5.1893.
(12)	" ..	1·0	"	" " "
		Typhoid-infected Unsterilised Thames + 1·0 *per cent.* NaCl.		
(13)	Flask 1 I ..	0·5	5 drops	Did not become turbid even in seven days.
(14)	" ..	1·0	"	" " "
(15)	Flask 1 R ..	0·5	"	" " "
(16)	" ..	1·0	"	" " "
		Typhoid-infected Unsterilised Thames + 3 *per cent.* NaCl.		
(17)	Flask 1 I ..	0·5	5 drops	Did not become turbid even in seven days.
(18)	" ..	1·0	"	" " "
(19)	Flask 1 R ..	0·5	"	" " "
(20)	" ..	1·0	"	" " "

Thus, the only saline water which gave a positive reaction with the phenol broth test was the one in which an addition of only 0·1 per cent. of salt had been made, and, even in this case, it was only the flask which had been kept at the winter temperature of 6—8° C. in the refrigerator, whilst the corresponding flask preserved at the summer temperature of 19° C. gave a negative result. Plate cultivations were made of the turbid broth-tube No. 11, and these yielded the characteristic typhoid colonies, which were further confirmed by potato growth, and negative results with the indol and gas-bubble tests. On referring to the results of phenol broth-culture (pp. 424—434) obtained with the corresponding waters to which no salt had been added, it will be seen that they present a great contrast to these, as both the incubator and refrigerator flasks of the typhoid-infected unsterilised Thames water gave a positive reaction, and were found to contain living typhoid bacilli.

From these examinations it appears then that on 29.5.1893, *or eighteen*

days after infection, the unsterilised Thames water still contained living typhoid bacilli, as did also the same water to which 0·1 *per cent. common salt had been added, and which had been preserved at* 6—8° C.*; on the other hand, in the unsterilised Thames waters to which* 1·0 *and* 3·0 *per cent. respectively of salt had been added, as well as in that which had only received* 0·1 *per cent. salt, but which had been kept at* 19° C., *the typhoid bacilli were no longer demonstrable.*

These saline waters were again examined by phenol broth-culture on 5.6.1893, with the following results:—

Examination by Phenol Broth-culture, 5.6.1893.

Number of broth tube.	Water used for cultivation with phenol broth.	Quantity of water taken in c.c.	Quantity of phenol solution added to 10 c.c. broth.	Remarks.
		Typhoid-infected Unsterilised Thames + 0·1 *per cent.* NaCl.		
(25)	Flask 1 I ..	0·5	3 drops	Turbid in 48 hours (pellicle on broth). Plates poured 9.6.1893.
(26)	„ ..	1·0	„	Turbid in 48 hours.
(31)	Flask 1 R ..	0·5	„	Turbid in 24 hours. Plates poured 6.6.1893.
(32)	„ ..	1·0	„	Turbid in 24 hours.
		Typhoid-infected Unsterilised Thames + 1 *per cent.* NaCl.		
(27)	Flask 1 I ..	0·5	3 drops	Turbid in 48 hours. Plates poured 9.6.1893.
(28)	„ ..	1·0	„	Turbid in 48 hours.
(33)	Flask 1 R ..	0·5	„	Turbid in 72 hours. Plates poured 9.6.1893.
(34)	„ ..	1·0	„	Turbid in 48 hours.
		Typhoid-infected Unsterilised Thames + 3 *per cent.* NaCl.		
(29)	Flask 1 I ..	0·5	3 drops	Not turbid until 8 days. Plates poured 13.6.1893.
(30)	„ ..	1·0	„	Turbid in 72 hours (pellicle on broth). Plates poured 9.6.1893.
(35)	Flask 1 R ..	0·5	„	Not turbid until 8 days. Plates poured 13.6.1893.
(36)	„ ..	1·0	„	Not turbid until 8 days.

Thus from the above the only water in which the presence of typhoid was still with certainty to be expected was the one to which 0·1 per cent. salt had been added, and which had been kept at 6—8° C. in the refrigerator, for this was the only one which gave a positive

result with the phenol broth in twenty-four hours, and although several others gave the reaction in forty-eight hours, it will be seen, by reference to p. 427, that even the uninfected unsterilised Thames water gave the turbidity in forty-eight hours when examined on the same day.

The plate cultivations made from the above turbid broth tubes gave the following results:—

Broth tube.

(31.) *Typhoid-infected Unsterilised Thames* + 1 *per cent.* NaCl, *Flask* 1 *Refrigerator.* (*Typhoid Present.*)

The colonies on the plate resembled those of typhoid, and they were confirmed by potato growth, negative indol, and gas-bubble tests.

(35.) *Typhoid-infected Unsterilised Thames* + 3 *per cent.* NaCl, *Flask* 1 *Refrigerator.* (*Typhoid Absent.*)

The colonies presenting any sort of resemblance to those of typhoid were transferred to potatoes, on which they gave a pink growth, therefore certainly not typhoid.

(29.) *Typhoid-infected Unsterilised Thames* + 3 *per cent.* NaCl, *Flask* 1 *Incubator.* (*Typhoid Absent.*)

Results similar to those obtained with No. 35 above.

(25.) *Typhoid-infected Unsterilised Thames* + 0·1 *per cent.* NaCl, *Flask* 1 *Incubator.* (*Typhoid Absent.*)

Results similar to those obtained with Nos. 35 and 29 above.

(27.) *Typhoid-infected Unsterilised Thames* + 1 *per cent.* NaCl, *Flask* 1 *Incubator.* (*Typhoid Absent.*)

The colonies presented some resemblance to those of typhoid, as did also the potato growths; on inoculating from latter, however, into gelatine tube, the gelatine underwent slow liquefaction, clearly showing that it was not really typhoid.

(33.) *Typhoid-infected Unsterilised Thames* + 1 *per cent.* NaCl, *Flask* 1 *Refrigerator.* (*Typhoid Absent.*)

The colonies which presented any sort of resemblance to those of typhoid were transferred to potatoes, on which they gave a pink growth, and, on inoculating into gelatine tubes, gas bubbles were formed, followed by liquefaction. Therefore, certainly not typhoid.

Broth tube.

(30.) *Typhoid-infected Unsterilised Thames* + 3 *per cent.* NaCl, *Flask* 1 *Incubator.* (*Typhoid Absent.*)

The colonies presented *some* resemblance to typhoid, so they were further examined by potato growth, indol, gas-bubble, and milk tests, in all of which the resemblance was maintained. Microscopically, the bacilli appeared smaller than typhoid, but the difference was not sufficient to place the matter beyond doubt. They were finally proved not to be typhoid by keeping the gelatine tube cultures for some time, when both the surface and depth growths were found to be distinctly yellow in colour. I have repeatedly encountered this same organism, which might be mistaken for typhoid in its earlier appearance in plate cultures, but it must clearly be understood that the resemblance is only a superficial one, and it was only submitted to so many tests because in these examinations anything bearing the slightest resemblance to typhoid was remanded for further enquiries.

These saline waters were again examined by phenol broth-culture on 13.6.1893, with the following results:—

Examination by Phenol Broth-culture on 13.6.1893.

Number of broth tube.	Water used for cultivation with phenol broth.	Quantity of water taken in c.c.	Quantity of phenol solution added to 10 c.c. broth.	Remarks.
		Typhoid-infected unsterilised Thames + 0·1 *per cent.* NaCl.		
(73)	Flask 1 I ..	1·0	3 drops	Turbid in 24 hours. Plates poured 14.6.1893.
(74)	" ..	0·5	"	Turbid in 48 hours.
(79)	Flask 1 R ..	1·0	"	Turbid in 24 hours. Plates poured 14.6.1893.
(80)	" ..	0·5	"	Turbid in 48 hours.
		Typhoid-infected unsterilised Thames + 1·0 *per cent.* NaCl.		
(75)	Flask 1 I ..	1·0	3 drops	Turbid in 24 hours. Plates poured 14.6.1893.
(76)	" ..	0·5	"	Turbid in 48 hours.
(81)	Flask 1 R ..	1·0	"	Turbid in 24 hours. Plates poured 14.6.1893.
(82)	" ..	0·5	"	Turbid in 24 hours.
		Typhoid-infected unsterilised Thames + 3 *per cent.* NaCl.		
(77)	Flask 1 I ..	1·0	3 drops	Turbid in 48 hours. Plates poured 15.6.1893.
(78)	" ..	0·5	"	Turbid in 48 hours.
(83)	Flask 1 R ..	1·0	"	Turbid in 48 hours.
(84)	" ..	0·5	"	Turbid in 48 hours. Plates poured 15.6.1893.

The six turbid broth tubes, indicated above as selected for further examination by gelatine plate culture, yielded the following results:—

Broth tube.

(73.) *Typhoid-infected Unsterilised Thames* + 0·1 *per cent.* NaCl, *Flask* 1 *Incubator.* (*Typhoid Absent.*)

The colonies yielded pink growths on potatoes and liquefied gelatine; they were therefore not those of typhoid.

(79.) *Typhoid-infected Unsterilised Thames* + 0·1 *per cent.* NaCl, *Flask* 1 *Refrigerator.* (*Typhoid Absent.*)

The colonies presented some resemblance to typhoid, as did also the growths on potatoes obtained from them. No indol reaction. On inoculating into gelatine tubes, the latter were found to very slowly liquefy on long keeping. On inoculating

from such a liquefied tube on to potatoes, the same typhoid-like growth was obtained. This organism, which was again subsequently met with, might easily lead to a false diagnosis of typhoid, unless the gelatine tubes were preserved for some time.

Broth tube.

(75.) *Typhoid-infected Unsterilised Thames* + 1 *per cent.* NaCl, *Flask* 1 *Incubator.* (*Typhoid Absent.*)

The colonies on transference to potatoes gave rise to light-brown growth, not like typhoid.

(82.) *Typhoid-infected Unsterilised Thames* + 1 *per cent.* NaCl, *Flask* 1 *Refrigerator.* (*Typhoid Absent.*)

The colonies liquefied the gelatine, and gave rise to easily visible growths on potatoes.

(78.) *Typhoid-infected Unsterilised Thames* + 3 *per cent.* NaCl, *Flask* 1 *Incubator.* (*Typhoid Absent.*)

The colonies gave rise to pink growths on potatoes; therefore certainly not typhoid.

(84.) *Typhoid-infected Unsterilised Thames* + 3 *per cent.* NaCl, *Flask* 1 *Refrigerator.* (*Typhoid Absent.*)

Same results as with No. 78 above.

Thus in the case of none of these saline waters could a diagnosis of typhoid bacilli be made on 13.6.1893; *by reference to p.* 433 *it will be seen also that on* 14.6.1893 *the typhoid bacilli were no longer demonstrable in the unsterilised Thames water to which no salt had been added.*

These saline waters were again examined by phenol broth-culture on 21.6.1893, with the following results:—

Examination by Phenol Broth-culture, 21.6.1893.

Number of broth tube.	Water used for cultivation with phenol broth.	Quantity of water taken in c.c.	Quantity of phenol solution added to 10 c.c. broth.	Remarks.
		Typhoid-infected Unsterilised Thames + 0·1 *per cent.* NaCl.		
(88)	Flask 1 I ..	1·0	3 drops	Turbid in 48 hours. Plates poured 23.6.1893.
(91)	„ 1 R ..	1·0	„	„ „ „ „
		Typhoid-infected Unsterilised Thames + 1·0 *per cent.* NaCl.		
(89)	Flask 1 I ..	1·0	3 drops	Turbid in 48 hours. Plates poured 23.6.1893.
(92)	„ 1 R ..	1·0	„	„ „ „ „
		Typhoid-infected Unsterilised Thames + 3·0 *per cent.* NaCl.		
(90)	Flask 1 I ..	1·0	3 drops	Turbid in 48 hours. Plates poured 23.6.1893.
(93)	„ 1 R ..	1·0	„	„ „ „ „

Thus, on this occasion, all the waters reacted in forty-eight hours with the phenol broth-solution. The following results were obtained on plate cultivating the turbid broth tubes:—

Broth tube.

(88.) *Typhoid-infected Unsterilised Thames* + 0·1 *per cent.* NaCl, *Flask* 1 *Incubator.* (*Typhoid Absent.*)

The colonies on transference to potatoes yielded light brown growths, which were certainly not due to typhoid.

(91.) *Typhoid-infected Unsterilised Thames* + 0·1 *per cent.* NaCl, *Flask* 1 *Refrigerator.* (*Typhoid Absent.*)

The potato growths from colonies somewhat resembled typhoid; there was also no indol reaction, but gelatine tubes were slowly liquefied by the organism, which was, therefore, not typhoid. (See similar experiences with No. 79, p. 443.)

(89.) *Typhoid-infected Unsterilised Thames* + 1 *per cent.* NaCl, *Flask* 1 *Incubator.* (*Typhoid Absent.*)

The potato growths from colonies were thick and brown in colour, quite unlike those of typhoid.

Broth tube.

(92.) *Typhoid-infected Unsterilised Thames* + 1 *per cent.* NaCl, *Flask* 1 *Refrigerator.* (*Typhoid Absent.*)

The potato growths from colonies were pink in colour, and, on inoculation into gelatine, liquefied it. Certainly not typhoid.

(90.) *Typhoid-infected Unsterilised Thames* + 3 *per cent.* NaCl, *Flask* 1 *Incubator.* (*Typhoid Absent.*)

The potato growths from colonies were pinkish-white in colour and much too conspicuous for typhoid.

(93.) *Typhoid-infected Unsterilised Thames* + 3 *per cent.* NaCl, *Flask* 1 *Refrigerator.* (*Typhoid Absent.*)

The potato growths from colonies were of a dirty white colour, and on inoculation into gelatine the latter was liquefied. Therefore, certainly not typhoid.

Thus, again, on this occasion, 21.6.1893, *it was found impossible to demonstrate the presence of typhoid bacilli in any of these saline waters.*

The saline waters were again examined by phenol broth-culture on 26.6.1893, with the following results:—

Examination by Phenol Broth-culture, 26.6.1893.

Number of broth tube.	Water used for cultivation with phenol broth.	Quantity of water taken in c.c.	Quantity of phenol solution added to 10 c.c. broth.	Remarks.
	Typhoid-infected Unsterilised Thames + 0·1 *per cent.* NaCl.			
(98)	Flask 1 I ..	1·0	3 drops	Turbid in 48 hours.
(101)	„ 1 R ..	„	„	„ „
	Typhoid-infected Unsterilised Thames + 1·0 *per cent.* NaCl.			
(99)	Flask 1 I ..	1·0	3 drops	Turbid in 48 hours.
(102)	„ 1 R ..	„	„	„ „
	Typhoid-infected Unsterilised Thames + 3·0 *per cent.* NaCl.			
(100)	Flask 1 I ..	1·0	3 drops	Turbid in 48 hours.
(103)	„ 1 R ..	„	„	„ „

These turbid broth tubes were submitted to plate cultivation on 28.6.1893, and the following results obtained:—

Broth tube.

(98.) *Typhoid-infected Unsterilised Thames* + 0·1 *per cent.* NaCl, *Flask* 1 *Incubator*, (*Typhoid Absent.*)

The plates exhibited liquefying and small colonies respectively. From the small colonies, potatoes were inoculated, a brown and slimy growth being obtained, on inoculation from which into gelatine tubes, a blue, fluorescent, non-liquefying growth resulted. Thus nothing like typhoid was present.

(101.) *Typhoid-infected Unsterilised Thames* + 0·1 *per cent.* NaCl, *Flask* 1 *Refrigerator.* (*Typhoid Absent.*)

The potato-growths obtained from the colonies bore some resemblance to typhoid, although the surface was rather too shining. On inoculating into gelatine tubes, it was found that very slow liquefaction took place. This was, therefore, the same organism which had been several times before met with in this flask. Under the microscope the bacilli also present some resemblance to typhoid, but they have squarer ends.

(99.) *Typhoid-infected Unsterilised Thames* + 1 *per cent.* NaCl, *Flask* 1 *Incubator.* (*Typhoid Absent.*)

On the plates there were small colonies forming surface expansions with green fluorescence ; these gave rise, on potatoes, to thick, greyish-brown growths quite unlike typhoid.

(102.) *Typhoid-infected Unsterilised Thames* + 1 *per cent.* NaCl, *Flask* 1 *Refrigerator.* (*Typhoid Absent.*)

The plates contained both liquefying and small colonies, the latter, on transferring to potatoes, gave strong, conspicuous, and flesh-coloured growths quite unlike typhoid.

(100.) *Typhoid-infected Unsterilised Thames* + 3 *per cent.* NaCl, *Flask* 1 *Incubator.* (*Typhoid Absent.*)

The plates contained small colonies forming surface expansions, which gave light brown growths on potatoes quite unlike typhoid.

(103.) *Typhoid-infected Unsterilised Thames* + 3 *per cent.* NaCl, *Flask* 1 *Refrigerator.* (*Typhoid Absent.*)

The plates contained liquefying and small colonies respectively, the potato-growths from the latter were strong, thick, waxy, and greyish-white, quite unlike typhoid.

Thus, on this occasion (26.6.1893) *again, the presence of typhoid bacilli could not be demonstrated in any of these saline waters.*

The final examination of these saline waters was made on 5.7.1893, with the following results :—

Examination by Phenol Broth-culture on 5.7.1893.

Number of broth tube.	Water used for cultivation with phenol broth.	Quantity of water taken. c.c.	Quantity of phenol solution added to 10 c.c. broth.	Remarks.
		Typhoid-infected Unsterilised Thames + 0·1 *per cent.* NaCl.		
(113)	Flask 1 I ..	1·0	3 drops	Turbid in 48 hours.
(123)	" ..	"	5 "	Not turbid in 6 days.
(116)	Flask 1 R ..	"	3 "	Turbid in 48 hours.
(126)	" ..	"	5 "	Turbid in 6 days. Plates poured.
		Typhoid-infected Unsterilised Thames + 1 *per cent.* NaCl.		
(114)	Flask 1 I ..	1·0	3 drops	Turbid in 48 hours.
(124)	" ..	"	5 "	Not turbid in 6 days.
(117)	Flask 1 R ..	"	3 "	Turbid in 24 hours. Plates poured 6.7.1893.
(127)	" ..	"	5 "	Not turbid in 6 days.
		Typhoid-infected Unsterilised Thames + 3 *per cent.* NaCl.		
(115)	Flask 1 I ..	1·0	3 drops	Turbid in 72 hours.
(125)	" ..	"	5 "	Not turbid in 6 days.
(118)	Flask 1 R ..	"	3 "	Turbid in 48 hours.
(128)	" ..	"	5 "	Not turbid in 6 days.

Plate cultivations were made of the turbid broth tubes with the following results :—

Broth tube.

(113.) *Typhoid-infected Unsterilised Thames* + 0·1 *per cent.* NaCl, *Flask* 1 *Incubator.* (*Typhoid Absent.*)

The plates had the appearance of being pure cultivations of an organism producing fluorescent expansion colonies without liquefaction. The colonies in which fluorescence was least conspicuous and which had, therefore, most chance of being typhoid, were transferred to potatoes, on which they yielded a greyish-brown growth sharply distinguishable from the potato and quite unlike typhoid.

(116.) *Typhoid-infected Unsterilised Thames* + 0·1 *per cent.* NaCl, *Flask* 1 *Refrigerator.* (*Typhoid Absent.*)

The plates exhibited a number of small colonies giving rise to surface expansions something like those of typhoid. These

colonies, on being transferred to potatoes, again yielded inconspicuous colourless growths very similar to those of typhoid. Negative results were also obtained with the milk, indol, and gas-bubble reactions. It was found, however, that the gelatine-tubes inoculated from these typhoid-like colonies underwent very slow liquefaction. This was, therefore, obviously the same organism which had been repeatedly obtained before from this flask, and the superficial resemblance to typhoid of which has been already referred to (see pp. 445, 447).

Broth tube.

(114.) *Typhoid-infected Unsterilised Thames* + 1 *per cent.* NaCl, *Flask* 1 *Incubator.* (*Typhoid Absent.*)

The plates contained fluorescent non-liquefying colonies. The less obviously fluorescent colonies on transference to potatoes yielded growths which were not sufficiently different from typhoid to decide, whilst the milk, indol, and gas-bubble tests were also negative. On inoculation into gelatine tubes, however, the latter became fluorescent.

(117.) *Typhoid-infected Unsterilised Thames* + 1 *per cent.* NaCl, *Flask* 1 *Refrigerator.* (*Typhoid Absent.*)

The plates exhibited a pure cultivation of bacillus, giving rise to small, cup-shaped, liquid colonies, probably *B. liquidus* (Percy Frankland), which is very frequently found to survive in the 3-drop phenol broth-cultures.

(115.) *Typhoid-infected Unsterilised Thames* + 3 *per cent.* NaCl, *Flask* 1 *Incubator.* (*Typhoid Absent.*)

The plates exhibited small, milk-drop colonies, with slight tendency to expand. Microscopic examination showed them to be due to bacilli thinner than the typhoid bacillus, and on potatoes they yielded light yellow, shining growths, unlike those of typhoid.

(118.) *Typhoid-infected Unsterilised Thames* + 3 *per cent.* NaCl, *Flask* 1 *Refrigerator.* (*Typhoid Absent.*)

The plates contained a number of liquefying colonies, apparently *B. liquidus* (Percy Frankland), also some small colonies, some of which gave rise to very small surface expansions and very small milk-drops. The potatoes inoculated from the milk-drop colonies yielded flesh-coloured growths, whilst those from the expansion colonies were not decisive.

Plates were again poured from these potatoes, and very small surface-expansion colonies again obtained, which, however, again were not like those of typhoid, and gelatine tubes inoculated from these yielded, in course of time, brown surface growths quite unlike typhoid.

The results of these experiments with the unsterilised Thames water, to which 0·1, 1, and 3 per cent. of common salt respectively had been added, are instructive in more ways than one. Thus:—

(1.) They show that the addition of the salt stimulated the growth and multiplication of some of the water bacteria to an enormous extent, the effect being the most marked with the largest proportion of salt (3 per cent.), whilst the water to which only 0·1 per cent. of salt was added, behaved almost exactly like the untreated Thames water.

(2.) This multiplication was, as usual, followed by decline, but the saline waters remained, even after six weeks, more densely, and with the larger proportion of salt much more densely, populated than the Thames water to which no salt was added.

(3.) As regards the effect of the salt addition on the typhoid bacilli present in the water, the experiments show that they were most prejudicially influenced. Thus whilst on the eighteenth day after infection the typhoid bacilli were easily demonstrable in the ordinary unsterilised Thames water to which no salt had been added, and also in that which had received 0·1 per cent. of salt and which had been kept at 6—8° C., they were not discoverable in any of the waters to which 1 and 3 per cent. salt had been added, nor in that which had received only 0·1 per cent. salt, but which had been kept at the summer temperature of 19° C.

(4.) In the case of the 3 per cent. salt addition, I am of opinion that the rapid disappearance of the typhoid bacilli is largely due to the direct action of the salt, whilst in the case of the smaller proportions it may also be due to the great multiplication of some of the common water bacteria.

Further experiments on the behaviour of typhoid bacilli in Thames water, to which salt had been added, were subsequently made (see pp. 530, *et seq.*), they proved entirely confirmatory of the results just recorded above, both as to the stimulation of the multiplication of the

water bacteria, and as to the more rapid disappearance of the typhoid bacilli. The directly prejudicial action of the salt on the typhoid bacillus was further demonstrated by the addition of salt to steam-sterilised Thames water containing typhoid bacilli.

Behaviour of the Typhoid Bacillus and of the B. coli communis *in Steam-sterilised Thames Water (First Series of Experiments).*

It will now be interesting to consider the behaviour of these bacilli in the precisely parallel series of experiments made with the same sample of Thames water which had been previously sterilised by steam. These experiments are of importance more especially because they enable us to ascertain whether the water contains the necessary food materials for these particular bacteria, as, owing to the absence of other forms, it is now possible to determine how the actual numbers of these bacteria are affected by residence in the water.

The infection and distribution of these steam-sterilised waters has already been described on pp. 410 and 411, so that I can pass at once to their subsequent examination, made both by gelatine plate and phenol broth-culture, at different intervals of time.

In the table (p. 452) the fate of the typhoid bacilli introduced into the steam-sterilised Thames water on 11.5.1893, is followed over a period of seventy-six days, and it will be seen that during this time their numbers underwent an almost continuous decline. Thus, whilst at the outset they were present to the number of, in round numbers, 70,000 per cub. cm., at the end of this period their presence was only just demonstrable by gelatine plate cultivation at all, and not more than from 6—12 were discoverable in 1 cub. cm. The most important feature in this chronicle of their deportment is the circumstance that they exhibited no multiplication or increase in numbers during their residence in the water, clearly showing, therefore, that the latter did not afford the nutriment and other conditions necessary for the proliferation of the typhoid bacilli. In fact, the decline from the commencement is an unbroken one, with the exception of the solitary observation of an increase from 27,000 on 22.5.1893, to 42,000 on 29.5.1893, in the case of the water maintained at a winter temperature. Whatever may have been the cause of this increase, it is not sufficiently great to be comparable with that extensive reproduction which takes place in the case of those bacteria which are the natural inhabitants of water. Moreover, that the water was not suited to the well-being of the typhoid bacilli was further testified to by the fact that the colonies on the gelatine plates became smaller, feebler, and more degenerate as time went on, until on the occasion of the last few examinations, they were, with difficulty, recognisable as typhoid colonies, and exhibited a marked disinclination to form the characteristic growths expanding over the surface of the gelatine.

Behaviour of Typhoid Bacillus in Steam-sterilised Thames Water, infected 11.5.1893.

Dates on which plate cultivations were made.	Particular flask employed.		Number of days plates were incubated.	Volume of water employed for plate cultivation.	Number of colonies obtained from 1 c.c. of water.	
	Incubator.	Refrigerator.			Incubator flasks.	Refrigerator flasks.
11.5.1893	Before subdivision		4	c.c. $\frac{1}{2}$ and $\frac{1}{5}$	74,000	
16.5.1893	1 I		3	$\frac{1}{5}$ and $\frac{1}{10}$	48,000	
,,		1 R	3	$\frac{2}{11}$ and $\frac{1}{11}$		51,000
22.5.1893	1 I		4	$\frac{1}{4}$ and $\frac{1}{12}$	27,000	
,,		1 R	3	1·0 and 0·5		27,000
29.5.1893	1 I		4	$\frac{3}{10}$ and $\frac{1}{10}$	24,000	
,,		1 R	4	$\frac{1}{5}$ and $\frac{1}{10}$		42,000
5.6.1893	1 I		4	$\frac{1}{5}$ and $\frac{1}{10}$	15,000	
,,		1 R	4	$\frac{1}{6}$ and $\frac{1}{15}$		40,000
6.7.1893	1 I		6	$\frac{1}{6}$ and $\frac{1}{15}$	5,000	
,,		1 R	8	$\frac{1}{5}$ and $\frac{1}{10}$		275
18.7.1893	1 I		8	$\frac{1}{2}$ and $\frac{1}{6}$	Only a few typhoid colonies.	
,,		1 R	8	$\frac{3}{5}$ and $\frac{1}{5}$		0
25.7.1893	1 I		4	1·0 and $\frac{3}{10}$	Only a few typhoid colonies.	
,,		1 R	6	1·0 and $\frac{3}{10}$	Only a few depth colonies, appeared to be typhoid.	
26.7.1893		1 R	5	3 and 4	Only 1 small depth colony, probably typhoid in a very degenerated state.	

As regards the effect of the higher or lower temperature at which the waters were maintained, it appeared throughout the greater part of the time that the typhoid bacilli in the flask, kept at the summer temperature (19° C.), suffered more rapid degeneration than those in the water, which was preserved at the winter temperature of 6° C., although quite at the last this relationship was reversed.

It is interesting to contrast, with the above results, the behaviour of the *B. coli communis* placed under precisely similar conditions in the same water and over the same period of time. The results of this comparative investigation are recorded in the following table :—

Behaviour of *Bacillus Coli communis* in Steam-sterilised Thames water, infected 11.5.1893.

Dates on which plate cultivations were made.	Particular flask employed.		Number of days plates were incubated.	Volume of water employed for plate cultivation.	Number of colonies obtained from 1 c.c. of water.	
	Incubator.	Refrigerator.			Incubator flasks.	Refrigerator flasks.
11.5.1893	Before subdivision		4	c.c. $\frac{3}{5}$ and $\frac{2}{5}$	69,000	
16.5.1893	1 I		3	$\frac{1}{5}$ and $\frac{1}{15}$	643,000	
"		1 R	3	$\frac{1}{50}$ and $\frac{1}{10}$		107,000
23.5.1893	1 I		3	$\frac{1}{5}$ and $\frac{1}{10}$	224,000	
"		1 R	3	$\frac{1}{5}$ and $\frac{1}{10}$		78,000
30.5.1893	1 I		3	$\frac{1}{5}$ and $\frac{1}{15}$	281,000	
"		1 R	3	$\frac{1}{5}$ and $\frac{1}{15}$		101,000
5.6.1893	1 I		4	$\frac{1}{5}$ and $\frac{1}{15}$	192,000	
"		1 R	4	$\frac{2}{11}$ and $\frac{1}{11}$		97,000
6.7.1893	1 I		6	$\frac{1}{5}$ and $\frac{1}{15}$	117,000	
"		1 R	6	$\frac{2}{5}$ and $\frac{1}{5}$		28,000
18.7.1893	1 I		4	$\frac{1}{5}$ and $\frac{1}{10}$	1,500	
"		1 R	4	$\frac{1}{5}$ and $\frac{1}{10}$		4,000
25.7.1893	1 I		4	$\frac{2}{5}$ and $\frac{2}{5}$	Coli colonies still abundantly present, but exact estimation difficult in consequence of air contamination of these plates.	
"		1 R	4	$\frac{1}{5}$ and $\frac{2}{10}$		5,000 (not contaminated)

From the above table it will be seen that the *B. coli communis* presents a great contrast to the typhoid bacillus in respect of its behaviour in this steam-sterilised Thames water. Thus, at the commencement of the experiment, both bacilli were present in about the same numbers, the typhoid-infected water containing 74,000, and the coli-infected water, 69,000 bacilli per 1 c.c. respectively, but whilst the subsequent numbers found in the typhoid-infected waters were invariably less than this initial number in the case of the coli-infected waters, the initial number was subsequently very greatly exceeded, the observed multiplication being greatest in the water kept at a summer temperature. This multiplication was afterwards followed by a corresponding decline, but even at the end of the seventy-five days over which the observations were extended, the number of the coli bacilli greatly exceeded that of the typhoid bacilli. It is, moreover, worthy of remark that the coli-infected water kept at 19° C., in which the most extensive multiplication took place, ultimately contained a smaller number of coli bacilli than the water maintained at 6° C., in which the multiplication had been less considerable.

The above results, obtained by the gelatine plate cultivation of the typhoid and coli-infected steam-sterilised Thames waters, were repeatedly confirmed and supplemented by the method of phenol broth-culture, thus :—

Examination by Phenol Broth-culture on 5.6.1893.

Number of broth tube.	Water used for cultivation with phenol broth.	Quantity of water taken in c.c.	Quantity of phenol solution added to 10 c.c. broth.	Remarks.
				Typhoid-infected Steam-sterilised Thames.
(53)	Flask 1 I ..	0·5	3 drops	Turbid in 24 hours.
(54)	" ..	1·0	"	" "
(55)	Flask 1 R ..	0·5	"	" "
(56)	" ..	1·0	"	" "
(61)	Flask 1 I ..	0·5	5 drops	Turbid in 24 hours. Plates poured 6.6.1893.
(62)	" ..	1·0	"	Turbid in 24 hours.
(63)	Flask 1 R ..	0·5	"	Very slight turbidity in 24 hours, pronounced turbidity in 48 hours.
(64)	" ..	1·0	"	Turbid in 24 hours.
				Coli-infected Steam-sterilised Thames.
(49)	Flask 1 I ..	0·5	3 drops	Turbid in 24 hours.
(50)	" ..	1·0	"	" "
(51)	Flask 1 R ..	0·5	"	" "
(52)	" ..	1·0	"	" "
(57)	Flask 1 I ..	0·5	5 drops	Turbid in 24 hours. Plates poured 6.6.1893.
(58)	" ..	1·0	"	Turbid in 24 hours.
(59)	Flask 1 R ..	0·5	"	Turbid in 24 hours. Plates poured 6.6.1893.
(60)	" ..	1·0	"	Turbid in 24 hours.

These examinations by phenol broth-culture show, therefore, that, on 5.6.1893, the typhoid-infected steam-sterilised Thames water reacted already in twenty-four hours with the test, irrespectively of whether 3 drops or 5 drops of phenol solution were added to the 10 c.c. of broth; whilst, by referring back to p. 427, it will be seen that the typhoid-infected unsterilised Thames water only reacted in twenty-four hours, even with the 3 drops of phenol solution, in the case of the water which had been kept at the winter temperature, whilst the summer temperature water only reacted after forty-eight hours. From these comparative tests it can be inferred, therefore, that, on the date in question, the typhoid bacilli were in a less vigorous condition in the unsterilised than in the sterilised water.

Of the plate cultivations made from the turbid broth tubes Nos. 61, 57, and 59, those from No. 61 yielded typical typhoid colonies which were confirmed by growth on potatoes and by negative results with the indol and gas-bubble tests; the plates from Nos. 57 and 59,

on the other hand, yielded the typical colonies of the *B. coli communis*, and these were further confirmed by growth in potatoes, and by positive results with the indol and gas-bubble tests.

The above phenol broth-culture tests were all made with 0·5 and 1·0 c.c. of the water, but on the same day (5.6.1893) some further experiments were made to incidentally determine whether much smaller volumes (a single drop) of water would react, and, if so, whether with equal rapidity. Thus—

Examination by Phenol Broth-culture of Small Quantities of Infected Water, 5.6.1893.

Number of broth tube.	Water used for cultivation with phenol broth.	Quantity of water taken in c.c.	Quantity of phenol solution added to 10 c.c. broth.	Remarks.
		Typhoid-infected Steam-sterilised Thames.		
(65)	Flask 1 I ..	1 drop	3 drops	Turbid in 24 hours.
(66)	" ..	"	5 "	Very slightly turbid in 24 hours; turbid in 48 hours.
(67)	Flask 1 R ..	"	3 "	Turbid in 24 hours.
(68)	" ..	"	5 "	Not turbid in 24 hours; turbid in 48 hours.
		Coli-infected Steam-sterilised Thames.		
(69)	Flask 1 I ..	1 drop	3 drops	Turbid in 24 hours.
(70)	" ..	"	5 "	" "
(71)	Flask 1 R ..	"	3 "	" "
(72)	" ..	"	5 "	" "

The interest attaching to the phenol broth examinations consists in the circumstance that the actual number of typhoid and coli bacilli present in the volumes of water used can be calculated from the results of the plate cultivations made on the same day (see pp. 452 and 454). Thus, it will be seen from the tables on pp. 456 and 457, that there was, in nearly all cases, practically no difference in the time which elapsed before the phenol broth tubes became turbid, irrespectively of whether 0·5 c.c., 1 c.c., or only 1 drop of the same water was employed; for, even in the 1 drop of the water, it is apparent from the plate cultivations (p. 452) that there must have been upwards of 1000 typhoid bacilli present, and a still larger number of coli bacilli in those waters infected with this bacillus.

Another examination by phenol broth-culture was made of the typhoid-infected steam-sterilised Thames water about one month later, on 5.7.1893 and on 6.7.1893, and for the last time on 25.7.1893. Thus—

Examination by Phenol Broth-culture.

Number of broth tube.	Water used for cultivation with phenol broth.	Quantity of water taken in c.c.	Quantity of phenol solution added to 10 c.c. broth.	Remarks.
		Typhoid-infected Steam-sterilised Thames.		
5.7.1893				
(110)	Flask 1 I ..	1 drop	3 drops	Turbid in 24 hours.
(112)	" ..	"	5 "	" 48 "
6.7.1893				
(129)	Flask 1 R ..	1 drop	3 drops	Remained clear.
(130)	" ..	"	5 "	" "
(131)	" ..	0·5 c.c.	3 "	Turbid in 72 hours.
(132)	" ..	"	5 "	" "
25.7.1893				
(199)	Flask 1 I ..	1·0 c.c.	3 drops	Turbid in 48 hours.
(200)	" 1 R ..	"	3 "	" "
		Coli-infected Steam-sterilised Thames.		
(195)	Flask 1 I ..	1·0 c.c.	3 drops	Turbid in 48 hours.
(196)	" 1 R ..	"	"	" "

From the above it will be seen that even on 25.7.1893, when the plate cultivations (see p. 452) were only yielding about twelve colonies per c.c., and these colonies of a very feeble and degenerate character, the phenol broth-cultures of 1 c.c. of the water still became turbid in forty-eight hours, and thus revealed the presence of living typhoid bacilli with the greatest facility.

On the other hand, when the number of typhoid bacilli in the water is small, it may very easily happen that a phenol broth tube now and again may fail to go turbid (as in the case of Broth tubes 443 and 444, see table above), and it is very necessary, therefore, to exercise great caution, and not to draw conclusions from a single observation, but only after a number of repeated trials.

The examinations by phenol broth-culture of these infected steam-sterilised Thames waters thus entirely substantiate the results arrived at

by the direct method of plate cultivation, and show that both the typhoid and coli bacilli were still present in a living state in this water, irrespectively of whether it had been preserved at a summer or a winter temperature, for a period of seventy-five days.

Behaviour of the Typhoid Bacillus and of the B. coli communis *in Thames Water sterilised by Filtration through Porous Porcelain. (First Series of Experiments.)*

The preparation and infection of this water has already been described (see pp. 410 and 411), and, as already indicated, the infected water was placed under precisely the same conditions of temperature, &c., as the steam-sterilised and unsterilised waters. In the periodical examination of this water the following results were obtained :—

Behaviour of Typhoid Bacillus in Thames Water sterilised by Filtration through Porous Porcelain, infected 11.5.1893.

Dates on which plate cultivations were made.	Particular flask employed.		Number of days plates were incubated.	Volume of water employed for plate cultivation.	Number of colonies obtained from 1 c.c. of water.	
	Incubator.	Refrigerator.			Incubator flask.	Refrigerator flasks.
11.5.1893	Before subdivision.		4	c.c. $\frac{1}{8}$ and $\frac{1}{5}$	75,000	
16.5.1893	1 I	1 R	9 6	$\frac{1}{6}$ and $\frac{1}{15}$ $\frac{1}{5}$ and $\frac{1}{10}$	0 (plates quite clear)	6700
22.5.1893	1 I	1 R	8 8	1·5 and 0·5 $\frac{3}{10}$ and $\frac{1}{10}$	0	0
29.5.1893	1 I	1 R	12 12	1·5 and 0·5 $\frac{5}{6}$ and $\frac{6}{15}$	0	0

The above results were most unexpected, for they show that, although as many as 75,000 typhoid bacilli per 1 c.c. were introduced into this water, they were entirely destroyed in five days at 19° C., and had undergone a very large reduction in number at 6—8° C., whilst in twelve days they were no longer discoverable in this water kept at the low temperature. On 2.6.1893 sterile broth was added to the flasks, which were then placed in the incubator at 38° C., but even this treatment did not lead to any revivification of the typhoid bacilli, which could neither be detected by plate cultivation nor phenol broth-culture.

The same rapid destruction of the *B. coli communis* was observed in this water, as will be seen from the following table:—

Behaviour of *B. coli communis* in Thames Water sterilised by Filtration through Porous Porcelain, infected 11.5.1893.

Dates on which plate cultivations were made.	Particular flask employed.		Number of days plates were incubated.	Volume of water employed for plate cultivation.	Number of colonies obtained from 1 c.c. of water.	
	Incubator.	Refrigerator.			Incubator flask.	Refrigerator flask.
11.5.1893	Before subdivision.		4	c.c. $\frac{1}{3}$ and $\frac{1}{6}$	83,000	
16.5.1893	1 I		9	$\frac{1}{5}$ and $\frac{1}{10}$	0	
		1 R	6	$\frac{1}{6}$ and $\frac{1}{12}$		8600
23.5.1893	1 I		7	$1\frac{4}{11}$ and $\frac{5}{11}$	0	
		1 R	7	$\frac{1}{3}$ and $\frac{1}{6}$		0
30.5.1893	1 I		11	1·5 and 0·5	0	
		1 R	11	$1\frac{4}{11}$ and $\frac{5}{11}$		0

Thus, in the case of the *B. coli communis* again, there was the same disappearance in five days of the bacilli in the water kept at 19° C., the great diminution in numbers in the same time in the water kept at 6—8° C., followed by complete disappearance of the bacilli in this water also by the twelfth day. Similar attempts made by the addition of sterile broth to resuscitate the bacilli in these waters also proved unavailing.

These results, showing that the typhoid and coli bacilli were more rapidly destroyed in the porcelain-filtered than in the unsterilised, and far more rapidly than in the steam-sterilised, Thames water, were so surprising that it was necessary to banish every suspicion of some accidental disturbing cause having arisen in these experiments.

The most obvious suggestion was that the filter itself might have introduced some antiseptic substance into the water. This was, however, highly improbable, as the filter in question had only been previously used for the similar sterilisation of Thames and Loch Katrine waters. In order to abolish this objection, however, the porcelain cylinder was thoroughly scrubbed externally with a tooth-brush, and then upwards of 30 litres of distilled water passed through it. The filter was then steam sterilised and employed for the filtration of some more of the same Thames water, which was infected with typhoid and coli as below. Thus—

(*a*.) *Typhoid Bacillus*.—20 needle-loops were taken from an agar cultivation of the typhoid bacillus of nine days age and introduced into 50 c.c. of steam-sterilised water; after thoroughly shaking, 3 c.c. of this water-attenuation were added to 750 c.c. of the porcelain-filtered Thames water.

(*b*.) *Bacillus coli communis*.—The infection was made in exactly the same way in every detail, the agar culture being also of the same age.

The waters thus infected with typhoid and coli respectively were subdivided into smaller flasks, some of which were placed as usual in the incubator at 19° C. and others in the refrigerator at 6—8° C. The following results were obtained on examination:—

Behaviour of the Typhoid Bacillus and *B. coli communis* in Thames Water Sterilised by Filtration through Porous Porcelain. Infected 15.6.1893.

Dates on which plate cultivations were made.	Particular flask employed.		Number of days plates were incubated.	Volume of water employed for plate cultivation.	Number of colonies obtained from 1 c.c. of water.	
	Incubator.	Refrigerator.			Incubator flask.	Refrigerator flask.
			Typhoid Bacillus.			
15.6.1893	Before subdivision.		8	c.c. $\frac{1}{5}$ and $\frac{1}{10}$	66,000	
20.6.1893	1 I		8	$\frac{1}{5}$ and $\frac{1}{10}$	0	
		1 R	8	$\frac{1}{5}$ and $\frac{1}{15}$		84
			B. coli communis.			
15.6.1893	Before subdivision.		8	c.c. $\frac{2}{15}$ and $\frac{1}{15}$	125,000	
20.6.1893	1 I		8	$\frac{1}{5}$ and $\frac{1}{10}$	0	
		1 R	8	$\frac{1}{5}$ and $\frac{1}{15}$		0

These results, therefore, entirely confirm those previously obtained, the disappearance of both the typhoid and coli bacilli being even still more rapid than on the former occasion.

Results of a similar character were also subsequently obtained with other waters sterilised by filtration (see pp. 479, 483, 502), in some of which, moreover, totally different filters, constructed of infusorial earth, were employed.

SECOND SERIES OF EXPERIMENTS.

The Behaviour of the Typhoid Bacillus and of the B. coli communis *in Loch Katrine Water.*

Having, in the first series of experiments, determined the behaviour of these bacilli in a typical calcareous surface water like that of the Thames, which receives the drainage from cultivated land, I proceeded in the next instance to carry out a somewhat similar series o experiments with Loch Katrine water, which may be taken as a type of an upland surface water derived almost exclusively from uncultivated land, and of a somewhat peaty character.

The sample of Loch Katrine was collected from a tap on the main in the Broomielaw, Glasgow, on June 30th, 1893.

Submitted to plate cultivation on the spot, it was found to contain 112 bacteria in 1 c.c., whilst on chemical analysis it yielded the following figures:—

Results of Analysis expressed in Parts per 100,000.

	Loch Katrine water uninfected.	Loch Katrine water infected with typhoid.	Loch Katrine water infected with *B. coli.*
Total solid matter	2·60		
Organic carbon } by combustion	0·185		
Organic nitrogen } by combustion	0·019		
Organic nitrogen (by Kjeldahl process)	0·013		
Ammonia (free)	0	0	
" (albuminoid)	0·006	0·013	
Oxygen consumed by organic matter	0·144	0·151	0·140
Nitrogen as nitrates and nitrites	0·006		
Total combined nitrogen	0·025		
Chlorine	0·65	0·65	0·65
Temporary hardness	0		
Permanent "	0·8		
Total "	0·8		
Proportion of organic carbon to organic nitrogen in suspended organic matter	8·14 : 1		

Infection of Loch Katrine Water with Typhoid and B. coli communis, 4.7.1893.

The cultures of the bacilli employed were on agar, and in both cases twenty-eight days old. In the case of the typhoid bacillus 40 needle-loops, and in that of the coli 25 loops, were taken from the surface of the agar, removing as little of the culture-material as possible, and introduced in each case into 50 c.c. of steam-sterilised water, which was then violently shaken to ensure disintegration of the bacterial masses. The experimental waters were then infected from these water-attenuations as follows:—

	Typhoid bacillus.	*Bacillus coli communis.*
Unsterilised Loch Katrine water	2,000 c.c. infected with 8 c.c. of water attenuation	1,000 c.c. infected with 3 c.c. of water attenuation.
Steam-sterilised Loch Katrine water	750 c.c. infected with 3 c.c. of water attenuation	750 c.c. infected with 2 c.c. of water attenuation.
Porcelain-filtered Loch Katrine water	750 c.c. infected with 3 c.c. of water attenuation	750 c.c. infected with 2 c.c. of water attenuation.

These infected waters, after thorough agitation, were then, as in previous experiments, subdivided amongst a number of small sterile conical flasks, plugged with sterile cotton-wool; in each case some of these flasks were placed in the incubator at 19° C., whilst others were kept in a refrigerator at 6—8° C. The uninfected unsterilised Loch Katrine water was also put into similar flasks, which were kept under precisely similar conditions for control.

1. *Bacteriological Examination of the Unsterilised Uninfected Loch Katrine Water.*

The control-waters were submitted to periodical examination both by gelatine-plate and phenol-broth culture, with the following results:—

Uninfected Unsterilised Loch Katrine Water. (First Series)

(Date of collection, June 30, 1893.)

Dates on which plate cultivations were made.	Particular flask employed.		Number of days plates were incubated.	Volume of water employed for plate cultivation.	Number of colonies obtained from 1 c.c. of water.	
	Incubator.	Refrigerator.			Incubator flask.	Refrigerator flask.
4.7.1893	Before subdivision.		3—4	c.c. $\frac{1}{30}$ and $\frac{1}{60}$	121 (Only few liquefying colonies.)	
10.7.1893	1 I		3—4	$\frac{1}{6}$, $\frac{1}{12}$, $\frac{1}{30}$, and $\frac{1}{60}$	925 (Only few liquefying colonies.)	
		1 R	3—5	$\frac{1}{6}$, $\frac{1}{12}$, $\frac{1}{30}$, and $\frac{1}{60}$		1150 (Only few liquefying colonies.)
17.7.1893	1 I		3—5	$\frac{1}{5}$, $\frac{1}{10}$, $\frac{1}{25}$, and $\frac{1}{50}$	716 (Few liquefying colonies.)	
		1 R	2—3	$\frac{1}{5}$, $\frac{1}{10}$, $\frac{1}{25}$, and $\frac{1}{50}$		1594 (Liquefying colonies more numerous.)
21.7.1893	1 I		4	$\frac{1}{5}$ and $\frac{1}{10}$	58 (Few liquefying colonies.)	
		1 R	3	$\frac{1}{6}$ and $\frac{1}{12}$		840 (Liquefying colonies more numerous.)

In this unsterilised uninfected Loch Katrine water it will be seen then that distinct, but only very restricted, multiplication took place, which was, as usual, followed by subsequent decline.

The examinations by phenol-broth culture of the uninfected unsterilised Loch Katrine water will be best considered along with the similar examinations made of the infected unsterilised waters (see p. 472, *et seq.*).

2. *Bacteriological Examination of the Infected Unsterilised Loch Katrine Water.*

The Loch Katrine waters, infected with typhoid and the *B. coli communis* respectively, were periodically examined, both by gelatine-plate and phenol-broth culture, with the following results :—

Typhoid-infected Unsterilised Loch Katrine Water. (First Series.) Infected 4.7.1893.

Dates on which plate cultivations were made.	Particular flask employed.		Number of days plates were incubated.	Volume of water employed for plate cultivation.	Number of colonies obtained from 1 c.c. of water.	
	Incubator.	Refrigerator.			Incubator flask.	Refrigerator flask.
4.7.1893	Before subdivision.		4	c.c. $\frac{1}{15}$ and $\frac{1}{50}$	690 (Few liquefying colonies.)	
10.7.1893	1 I		3	$\frac{1}{25}$ and $\frac{1}{50}$	1425 (Numerous liquefying colonies; no colonies unmistakably like typhoid.)	
		1 R	3	$\frac{1}{25}$ and $\frac{1}{50}$		8125 (Very numerous liquefying colonies; no colonies unmistakably like typhoid.)
17.7.1893	1 I		2	$\frac{1}{30}$ and $\frac{1}{50}$	2250 (Very numerous liquefying colonies; no surface colonies with marked resemblance to typhoid.)	
		1 R	2—3	$\frac{1}{25}$ and $\frac{1}{50}$		5000 (Very numerous liquefying colonies; no surface colonies with marked resemblance to typhoid.)
21.7.1893	1 I		2	$\frac{2}{11}$ and $\frac{1}{11}$	4400 (Very numerous liquefying colonies.)	
		1 R	2—3	$\frac{1}{5}$ and $\frac{1}{10}$		4105 (Very numerous liquefying colonies.)

In this case there was a very considerable increase in the total number of bacteria present, due to the extensive multiplication of the water bacteria, as shown by the great increase in the number of liquefying colonies. As this multiplication is much more marked than in the uninfected water, it must obviously have been promoted by the small quantity of organic matter unavoidably introduced along with the typhoid bacilli.

Similarly the plate cultivations of the unsterilised Loch Katrine water infected with the *B. coli communis* yielded the following results:—

Coli-infected Unsterilised Loch Katrine Water.

Dates on which plate cultivations were made.	Particular flask employed.		Number of days plates were incubated.	Volume of water employed for plate cultivation.	Number of colonies obtained from 1 c.c. of water.	
	Incubator.	Refrigerator.			Incubator flask.	Refrigerator flask.
4.7.1893	Before subdivision.		3—4	c.c. $\frac{1}{5}$, $\frac{1}{10}$, $\frac{1}{20}$, $\frac{1}{50}$	3350 (Only few liquefying colonies; numerous surface colonies like those of typhoid or coli.)	
11.7.1893	1 I		3	$\frac{1}{5}$, $\frac{1}{15}$, $\frac{1}{25}$, $\frac{1}{50}$	1900 (Few liquefying colonies; only few surface colonies like typhoid or coli.)	
		1 R	3	$\frac{2}{5}$, $\frac{1}{5}$, $\frac{1}{25}$, $\frac{1}{50}$		5400 (Numerous liquefying colonies, no surface colonies like typhoid or coli.)
18.7.1893	1 I		4	$\frac{1}{5}$ and $\frac{1}{10}$	63 (Very few colonies at all; one surface colony just like typhoid or coli.)	
		1 R	3	$\frac{1}{5}$ and $\frac{1}{10}$		3825 (Few liquefying colonies, some surface colonies like typhoid or coli.)
21.7.1893	1 I		3	$\frac{2}{5}$ and $\frac{1}{5}$	72 (Few liquefying colonies; no surface colonies like typhoid or coli.)	
		1 R	3	$\frac{1}{5}$ and $\frac{1}{15}$		4029 (A number of liquefying colonies, and some surface colonies like typhoid or coli.)

In the water, therefore, kept at the summer temperature there was a continuous decline in the total number of bacteria, nor was there, apparently, any multiplication of the water forms; whilst in the water kept at a winter temperature, not only was there a slight numerical increase, but also, obviously, a considerable multiplication of the water-bacteria as evidenced by the increase in the number of liquefying colonies.

In the following tables are recorded the results of the examinations by phenol-broth culture of the several unsterilised Loch Katrine waters, both infected and uninfected:—

Examination of Unsterilised Loch Katrine Waters (First Series) by Phenol Broth-culture, 8.7.1893.

Number of broth tube.	Water used for cultivation with phenol broth.	Quantity of water taken in c.c.	Quantity of phenol solution added to 10 c.c. broth.	Remarks.
		Uninfected Unsterilised Loch Katrine.		
(120)	Flask 1 I ..	1·0	3 drops	Turbid in 48 hours.
(129)	" ..	"	5 "	Did not go turbid.
(121)	Flask 1 R ..	"	3 "	Turbid in 48 hours.
(130)	" ..	"	5 "	Turbid in 48 hours. Plates poured 10.7.1893.
		Typhoid-infected Unsterilised Loch Katrine.		
(122)	Flask 1 I ..	1·0	3 drops	Turbid in 48 hours.
(131)	" ..	"	5 "	Turbid in 48 hours. Plates poured 10.7.1893.
(123)	Flask 1 R ..	"	3 "	Turbid in 48 hours.
(132)	" ..	"	5 "	Turbid in 48 hours. Plates poured 10.7.1893.
		Coli-infected Unsterilised Loch Katrine.		
(126)	Flask 1 I ..	1·0	3 drops	Turbid in 24 hours.
(135)	" ..	"	5 "	Turbid in 24 hours. Plates poured 9.7.1893.
(127)	Flask 1 R ..	"	3 "	Turbid in 24 hours.
(136)	" ..	"	5 "	Turbid in 24 hours. Plates poured 9.7.1893.

From the above table it will be seen that all the waters rendered the phenol broth-tubes turbid, those infected with the *B. coli com-*

munis in twenty-four hours, the uninfected and typhoid-infected waters in forty-eight hours.

Of the plate cultivations prepared from these turbid broth-tubes, it need only be stated that the plates from the tubes which had gone turbid with typhoid-infected water, yielded characteristic typhoid colonies which satisfied all the several confirmatory tests; similarly the plates prepared from those broth-tubes which had been rendered turbid by coli-infected water, yielded the characteristic colonies of the *B. coli communis*, and also satisfied the various confirmatory tests. On the other hand, those phenol broth-tubes which had become turbid through the uninfected water, yielded colonies on the plates which were small in the depth, and formed small pin-heads on the surface of the gelatine, but did not give rise to the characteristic expansions, whilst on transferring these to potatoes, strong, highly-raised, greyish growths, much more conspicuous than those of typhoid, were obtained.

Thus on July 8, 1893, *four days after infection, both the typhoid bacillus and the* B. coli communis *were proved to be still alive in the unsterilised Loch Katrine water.*

The second examination by phenol broth-culture was made on July 15, 1893, or eleven days after infection, with the following results:—

Examination of Unsterilised Loch Katrine Waters by Phenol Broth-culture, 15.7.1893.

Number of broth tube.	Water used for cultivation with phenol broth.	Quantity of water taken in c.c.	Quantity of phenol solution added to 10 c.c. broth.	Remarks.
		Uninfected Unsterilised Loch Katrine.		
(140)	Flask 1 I ..	1·0	3 drops	Turbid in 48 hours. Plates poured 18.7.1893.
(146)	„ ..	„	5 „	Did not go turbid.
(141)	Flask 1 R ..	„	3 „	Turbid in 48 hours. Plates poured 18.7.1893.
(147)	„ ..	„	5 „	Did not go turbid.
		Typhoid-infected Unsterilised Loch Katrine.		
(142)	Flask 1 I ..	1·0	3 drops	Turbid in 72 hours. Plates poured 18.7.1893.
(148)	„ ..	„	5 „	Did not go turbid.
(143)	Flask 1 R ..	„	3 „	Turbid in 24 hours. Plates poured 18.7.1893.
(149)	„ ..	„	5 „	Did not go turbid.
		Coli-infected Unsterilised Loch Katrine.		
(144)	Flask 1 I ..	1·0	3 drops	Turbid in 24 hours.
(150)	„ ..	„	5 „	Turbid in 48 hours. Plates poured 17.7.1893.
(145)	Flask 1 R ..	„	3 „	Turbid in 24 hours.
(151)	„ ..	„	5 „	Turbid in 48 hours.

Of the plate cultivations made from the above turbid broth-tubes, it will be sufficient to say:—

1. That from the uninfected water only liquefying colonies were obtained, probably *B. liquidus* (Percy Frankland); as already mentioned, this organism is very frequently obtained in phenol broth-cultivations in which only 3 drops of phenol solution has been added.

2. The plates from the phenol broth-tube which had only been rendered turbid in seventy-two hours by the incubator flask of the typhoid-infected water, yielded also only liquefying colonies, and nothing like typhoid colonies was discoverable on the plates. The corresponding broth-tube from the refrigerator flask, on the other hand, which had become turbid in twenty-four hours, yielded numerous small and expansion colonies which were undoubtedly due to typhoid.

3. The broth-tube, which had been rendered turbid in twenty-four hours with coli-infected water, yielded plates containing numerous depth and surface-expansion colonies, which were undoubtedly those of the *B. coli communis.*

Thus from these examinations it was apparent that on July 15, 1893, *or eleven days after infection, the* Bacillus coli communis *was still alive in the unsterilised Loch Katrine water, as was also the typhoid bacillus in similar water which had been kept at the winter temperature of* 6—8° C., *whilst in the water kept at a summer temperature of* 19° C. *the typhoid bacillus was no longer discoverable.*

Another examination was made of these uninfected and infected unsterilised Loch Katrine waters on July 21, 1893, with the following results:—

Examination of Unsterilised Loch Katrine Waters by Phenol Broth-culture, 21.7.1893.

Number of broth tube.	Water used for cultivation with phenol broth.	Quantity of water taken in c.c.	Quantity of phenol solution added to 10 c.c. broth.	Remarks.
		Uninfected Unsterilised Loch Katrine.		
(175)	Flask 1 I ..	1·0	3 drops	Only turbid after 8 days.
(176)	„ 1 R..	„	„	Only turbid after 4 days.
		Typhoid-infected Unsterilised Loch Katrine.		
(177)	Flask 1 I ..	1·0	3 drops	Turbid in 4 days. Plates poured 25.7.1893.
(178)	„ 1 R..	„	„	Turbid in 48 hours. Plates poured 23.7.1893.
		Coli-infected Unsterilised Loch Katrine.		
(179)	Flask 1 I ..	1·0	3 drops	Turbid in 48 hours. Plates poured 23.7.1893.
(180)	„ 1 R..	„	„	Turbid in 48 hours. Plates poured 23.7.1893.

The plate cultivations made from the above turbid broth-tubes yielded the same results as those on July 15, 1893; thus no typhoid colonies were obtained on the plates from broth-tube No. 177, whilst they were easily discoverable and confirmed on the plates from broth-tube No. 178; again the colonies of the *B. coli communis* were

readily detected and confirmed on the plates from both broth-tubes Nos. 179 and 180.

From these examinations it was evident, therefore, that the B. coli communis *was still alive in the unsterilised Loch Katrine waters (kept both at winter and summer temperature) on July* 21, 1893, *or seventeen days after infection; the typhoid bacillus was also still alive in the similar water kept at the winter temperature* (6—8° *C.*), *whilst it was again, as on the previous occasion* (July 15, 1893), *proved to be extinct in the same water kept at the summer temperature of* 19° *C.*

3. *Bacteriological Examination of the Infected Sterilised Loch Katrine Waters.*

With the preceding results must now be compared the behaviour of the typhoid bacillus and the *B. coli communis* in the Loch Katrine water which had been previously sterilised by steam and by filtration through porous porcelain respectively.

These infected sterile waters were, as before, intended to show whether these bacilli are capable of multiplication or not in water of this character when the disturbing influence of the simultaneous presence of other micro-organisms is removed.

Typhoid-infected Steam Sterilised Loch Katrine Water. (First Series.)

Dates on which plate cultivations were made.	Particular flask employed.		Number of days plates were incubated.	Volume of water employed for plate cultivation.	Number of colonies obtained from 1 c.c. of water.	
	Incubator.	Refrigerator.			Incubator flask.	Refrigerator flask.
4.7.1893	Before subdivision.		6	c.c. $\frac{1}{4}$ and $\frac{1}{12}$	720	
10.7.1893	1 I 	 1 R	7 7	$\frac{1}{3}$ and $\frac{1}{6}$ $\frac{1}{4}$ and $\frac{1}{12}$	77	228
17.7.1893	1 I 	 1 R	7--8 8	1·0 and $\frac{3}{10}$ $\frac{5}{6}$ and $\frac{1}{4}$	15	29
21.7.1893	1 I 	 1 R	5 5	1·0 and 0·5 $\frac{5}{6}$ and $\frac{5}{12}$	0	1
25.7.1893	1 I 	 1 R	6 6	$\frac{5}{6}$ and $\frac{5}{12}$ 1·0 and 0·5	0	2

Thus in this steam-sterilised Loch Katrine water the typhoid bacilli underwent rapid degeneration, the rate of their decline being more rapid at the summer than at the winter temperature; for at the higher temperature they were no longer demonstrable seventeen days after infection, whilst at the lower temperature they were still just discoverable even after twenty-one days.

Essentially similar was the behaviour of the typhoid bacilli in the Loch Katrine water which had been sterilised by filtration through porous porcelain, thus:—

Typhoid-infected Porcelain-filtered Loch Katrine Water. (First Series.)

Dates on which plate cultivations were made.	Particular flask employed.		Number of days plates were incubated.	Volume of water employed for cultivation.	Number of colonies obtained from 1 c.c. of water.	
	Incubator.	Refrigerator.			Incubator flask.	Refrigerator flask.
4.7.1893	Before subdivision.		6	c.c. $\frac{3}{11}$ and $\frac{1}{11}$	615	
10.7.1893	1 I		7	$\frac{3}{13}$ and $\frac{1}{13}$	130	
		1 R	7	$\frac{3}{10}$ and $\frac{1}{10}$		345
17.7.1893	1 I		8	$\frac{5}{6}$ and $\frac{1}{4}$	50	
		1 R	8	$\frac{5}{6}$ and $\frac{1}{6}$		100
21.7.1893	1 I		5	$\frac{10}{11}$ and $\frac{5}{11}$	1 (?) Plate contaminated.	
		1 R	5	$\frac{5}{6}$ and $\frac{5}{12}$		2 (?) Plate much contaminated.
25.7.1893	1 I		6	1·0 and 0·5	0 Not contaminated.	
		1 R	6	1·0 and 0·5		20 Not contaminated.

Thus again in the Loch Katrine water, sterilised by filtration through porous porcelain, the typhoid bacilli underwent rapid degeneration, more especially in the water which was preserved at the summer temperature, in which they were no longer found by plate cultivation twenty-one days after infection, whilst in the same water kept at the winter temperature they were still easily recognisable, although in greatly diminished numbers, on that day.

It is particularly noteworthy that the behaviour of the typhoid bacilli was practically identical in this Loch Katrine water, irrespectively of whether it was employed in the unsterilised or in the sterilised condition, and irrespectively of whether the sterilisation was effected by steam or by filtration through porous porcelain.

In all cases, moreover, the effect of temperature on the typhoid bacillus was very marked, the longevity being much greater in the Loch Katrine water, unsterilised or sterilised, kept at the winter than in that kept at the summer temperature.

The behaviour of the *B. coli communis* in these sterilised L. Katrine waters is recorded in the following tables:—

Coli-infected Steam-sterilised Loch Katrine Water.

Dates on which plate cultivations were made.	Particular flask employed.		Number of days plates were incubated.	Volume of water employed for plate cultivation.	Number of colonies obtained from 1 c.c. of water.	
	Incubator.	Refrigerator.			Incubator flask.	Refrigerator flask.
				c.c.		
4.7.1893	Before subdivision.		6	$\frac{1}{4}$ and $\frac{1}{12}$	2,180	
11.7.1893	1 I		6	$\frac{3}{11}$ and $\frac{1}{11}$	0	
		1 R	6	$\frac{1}{4}$ and $\frac{1}{12}$		162
18.7.1893	1 I		7	2·0 and 1·0	1	
		1 R	7	2·0 and 1·0		49
21.7.1893	1 I		5	2·0 and 1·0	0	
		1 R	5	$\frac{3}{5}$ and $\frac{1}{5}$		0
25.7.1893	1 I		6	2·0 and 1·0	0	
		1 R	6	$\frac{1}{2}$ and $\frac{1}{6}$		0
26.7.1893	1 I		5	4·0 and 3·0	0	
		1 R	5	4·0 and 2·0		0

Thus in the steam-sterilised L. Katrine water, the B. coli communis *disappeared in a surprisingly short time, being no longer demonstrable on the* 17*th day after infection. In this case also the decline was more rapid in the water kept at a summer than in that at a winter temperature.*

Very similar again was the behaviour of the *B. coli communis* in the L. Katrine water previously sterilised by filtration through porous porcelain, thus :—

Coli-infected Porcelain-filtered Loch Katrine Water.

Dates on which plate cultivations were made.	Particular flask employed.		Number of days plates were incubated.	Volume of water employed for plate cultivation.	Number of colonies obtained from 1 c.c. of water.	
	Incubator.	Refrigerator.			Incubator flask.	Refrigerator flask.
4.7.1893	Before subdivision.		6	c.c. $\frac{3}{13}$ and $\frac{1}{13}$	1,900	
11.7.1893	1 I		6	$\frac{3}{10}$ and $\frac{1}{10}$	0	
		1 R	6	$\frac{3}{10}$ and $\frac{1}{10}$		15
18.7.1893	1 I		7	1·0 and 0·5	0	
		1 R	7	2·0 and 1·0		8
21.7.1893	1 I		5	2·0 and 1·0	0	
		1 R	5	2·0 and 1·0		1
25.7.1893	1 I		6	2·0 and 1·0	4	
		1 R	6	2·0 and 1·0		1
26.7.1893	1 I		5	4·0 and 3·0	0	

Thus again in the case of this porcelain-filtered L. Katrine water there was the same rapid disappearance of the introduced B. coli communis.

It is particularly remarkable that the B. coli communis *has disappeared more rapidly in both these sterile L. Katrine waters than in the unsterilised.*

Similar evidence of the disappearance of the typhoid and coli bacilli in these sterile L. Katrine waters is afforded by the results of the several examinations by phenol broth-culture, thus:—

Examinations of Typhoid-infected Sterilised Loch Katrine Waters by Phenol Broth-culture.

Date and number of broth tube.	Water used for cultivation with phenol broth.	Volume of water taken in c.c.	Quantity of phenol solution added to 10 c.c. broth.	Remarks.
8.7.1893	*Typhoid-infected Steam-sterilised Loch Katrine.*			
(137)	Flask 1 R..	1 drop	3 drops	Turbid in 24 hours.
17.7.1893				
(162)	Flask 1 I ..	0·5	3 drops	Turbid in 24 hours.
(163)	„ ..	1·0	„	„ „
20.7.1893				
(172)	Flask 1 I ..	0·5	3 drops	Did not go turbid.
(173)	„ ..	1·0	„	„ „
25.7.1893				
(207)	Flask 1 I ..	1·0	3 drops	Turbid in 4 days.
(208)	„ 1 R..	„	„	Did not go turbid.
	Typhoid-infected Porcelain-filtered Loch Katrine.			
(209)	Flask 1 I ..	1·0	3 drops	Did not go turbid.
(210)	„ 1 R..	„	„	Turbid in 48 hours.
26.7.1893	*Typhoid-infected Steam-sterilised Loch Katrine.*			
(220)	Flask 1 I ..	1·0	3 drops	Did not go turbid.
(221)	„ 1 R..	„	„	„ „

Examinations of Coli-infected Sterilised Loch Katrine Waters by Phenol Broth-culture.

Date and number of broth tube.	Water used for cultivation with phenol broth.	Volume of water taken in c.c.	Quantity of phenol solution added to 10 c.c. broth.	Remarks.
21.7.1893.	*Coli-infected Steam-sterilised Loch Katrine.*			
(195)	Flask 1 I ..	2·0	3 drops	Did not become turbid.
(196)	" ..	1·0	"	" "
	Coli-infected Porcelain-filtered Loch Katrine.			
(197)	Flask 1 I ..	2·0	3 drops	Did not become turbid.
(198)	" ..	1·0	"	" "
(199)	Flask 1 R ..	2·0	"	Turbid in 48 hours.
(200)	" ..	1·0	"	Did not become turbid.
25.7.1893.	*Coli-infected Steam-sterilised Loch Katrine.*			
(203)	Flask 1 I ..	1·0	3 drops	Did not become turbid.
(204)	" 1 R ..	1·0	"	" "
	Coli-infected Porcelain-filtered Loch Katrine.			
(205)	Flask 1 I ..	1·0	3 drops	Turbid in 48 hours.
(206)	" 1 R ..	1·0	"	" "
26.7.1893. (222)	Flask 1 I ..	1·0	3 drops	Did not become turbid.

These examinations by phenol broth-culture substantiate the results obtained by gelatine plates, and show that only a very small number of the bacilli were still living in the waters on the later dates. Thus, in the case of broth-tubes Nos. 199 and 200, it is evident that in No. 199, in which 2 c.c. of water were employed, at least one living *B. coli communis* was introduced, for the broth-tube became turbid; whilst in No. 200, in which only 1 c.c. of the same water was employed, no living bacillus can have been introduced, as the broth-tube did not become turbid. On referring to the table of gelatine plate examinations (p. 483) it will be seen that on the same day (21.7.1893) in the same water there was found only one *B. coli communis* colony per 1 c.c., so that it might easily happen that any particular 1 c.c. of the water might not contain any bacillus, as was apparently the case in the 1 c.c. of this water added to the phenol broth-tube No. 200.

It is in this way that particular interest attaches to a comparison of the results obtained by gelatine plate and phenol broth-culture in the case of these infected sterile waters, as the two methods can be made to control each other, whilst in the case of the infected unsterilised waters the method of phenol broth-culture has to be exclusively relied on for the detection of the typhoid and coli bacilli.

Behaviour of the Typhoid Bacillus in the Loch Katrine Water. (Second Series of Experiments.)

In the first series of experiments with the L. Katrine water recorded above, the number of typhoid bacilli initially introduced was so small that it would obviously not be possible to directly compare the results with those previously obtained with Thames water in which a much larger number of typhoid bacilli were initially introduced, as I have found in previous investigations of the same kind that one of the factors determining the longevity of pathogenic bacteria placed in water, or for the matter of that placed in any unfavourable surroundings, is the absolute number in which they are present. In other words, amongst, for instance, 1,000 bacteria taken from a given source there may be *some individuals* which will resist a particular adverse influence, whilst amongst 10 bacteria taken from the same source there may be *none* capable of resisting the adverse influence in question.

When, therefore, I found that such a small number of typhoid bacilli had been introduced into the L. Katrine water in the first series of experiments, I immediately started a second series of experiments with the same water, but introducing a much larger number of typhoid bacilli.

In this second series of Loch Katrine experiments, which were begun on 7.7.1893, or three days after the first, only unsterilised Loch Katrine water was infected with typhoid, thus :—

Infection of L. Katrine Water in Second Series of Experiments.—25 needle-loops were taken from the surface of an agar-culture of the typhoid bacillus, 11 days old, and introduced into 20 c.c. of steam-sterilised tap-water, which was then violently shaken for 15 minutes; 10 c.c. of this water-attenuation were then added to 1500 c.c. of unsterilised L. Katrine water. After thorough mixture this was divided up amongst a number of sterilised flasks plugged with sterile cotton-wool, which were placed in the incubator (19° C.) and refrigerator (6—8° C.) respectively. Control flasks containing the same unsterilised L. Katrine water, but uninfected, were placed under precisely similar conditions.

The results of bacteriological examination of the uninfected control L. Katrine water are given in the following table :—

Unsterilised Uninfected Loch Katrine Water. (Second Series.)

Dates on which plate cultivations were made.	Particular flask employed.		Number of days plates were incubated.	Volume of water employed for plate cultivation.	Number of colonies obtained from 1 c.c. of water.	
	Incubator.	Refrigerator.			Incubator flask.	Refrigerator flask.
7.7.1893	Before subdivision.		3	c.c. $\frac{2}{11}$ and $\frac{1}{11}$	410	
11.7.1893	1 I		3	$\frac{1}{6}$, $\frac{1}{25}$, $\frac{1}{30}$, $\frac{1}{60}$	4550	
		1 R	3	$\frac{1}{5}$, $\frac{1}{10}$, $\frac{1}{25}$, $\frac{1}{50}$		412
18.7.1893	1 I		6	$\frac{2}{5}$ and $\frac{1}{5}$	39	
		1 R	3	$\frac{1}{5}$ and $\frac{1}{10}$		335
21.7.1893	1 I		3	$\frac{2}{15}$ and $\frac{1}{15}$	114	
		1 R	3—4	$\frac{1}{5}$ and $\frac{1}{12}$		657
					N.B.—Only a small number of liquefying colonies on any of the above plates.	

Typhoid-infected Unsterilised Loch Katrine Water. (Second Series.)

Dates on which plate cultivations were made.	Particular flask employed.		Number of days plates were incubated.	Volume of water employed for plate cultivations.	Number of colonies obtained from 1 c.c. of water.	
	Incubator.	Refrigerator.			Incubator flask.	Refrigerator flask.
7.7.1893	Before subdivision.		3	c.c. $\frac{1}{30}$ and $\frac{1}{50}$	441,000	
11.7.1893	1 I		3	$\frac{1}{25}$ and $\frac{1}{50}$	298,000 (Plates badly liquefied, so that numbers somewhat uncertain, and doubtless under-estimated.)	
		1 R	3	$\frac{1}{25}$ and $\frac{1}{50}$		250,000 (Plates also badly liquefied. This liquefaction indicates that the water-bacteria must have undergone great multiplication.)
18.7.1893	1 I		4	$\frac{1}{50}$	8250 (A few surface colonies, doubtless typhoid.)	
		1 R	3	$\frac{1}{25}$ and $\frac{1}{50}$		253,000 (Very numerous surface colonies, doubtless typhoid.)
					In the plates from both waters a number of liquefying colonies present.	
21.7.1893	1 I		2	$\frac{1}{30}$ and $\frac{1}{50}$	3800 (Several expansion colonies, doubtless typhoid.)	
		1 R	3	$\frac{1}{25}$ and $\frac{1}{50}$		8000 (Several expansion colonies, doubtless typhoid.)
					The plates from both waters contained a number of liquefying colonies.	

Thus in these uninfected L. Katrine waters only comparatively slight multiplication took place, and at no time were there more than a few colonies causing liquefaction of the gelatine.

With these must now be compared the L. Katrine water infected with typhoid, the results obtained with which are recorded in the table (p. 488).

These examinations show that although the water-bacteria present in the unsterilised L. Katrine water must have undergone considerable multiplication, as shown by the great increase in the liquefying colonies, yet this multiplication did not by any means keep pace with the decrease in the number of typhoid bacilli; for the total number of colonies on the successive plates underwent continuous decline.

The actual proof of the persistence of the typhoid bacillus in these waters had of course to be furnished by the method of phenol broth-culture. Thus

Examination of Unsterilised Loch Katrine Waters (Second Series) by Phenol Broth-culture.

Number of broth tube.	Water used for cultivation with phenol broth.	Volume of water taken in c.c.	Quantity of phenol solution added to 10 c.c. broth.		Remarks.
				Typhoid-infected Unsterilised Loch Katrine (Second Series).	
8.7.1893					
(124)	Flask 1 I ..	1·0	3 drops	Turbid in 48 hours.	On plate cultivation the tubes Nos. 133 and 134 yielded typical typhoid colonies, which were further confirmed by usual tests. *Typhoid present.*
(133)	" ..	"	5 "	" "	
(125)	Flask 1 R ..	"	3 "	" "	
(134)	" ..	"	5 "	" "	
15.7.1893					
(154)	Flask 1 I ..	"	3 "	Turbid in 72 hours.	On plate cultivation the tubes Nos. 155, 158 and 159, yielded typical typhoid colonies, which were further confirmed by usual tests. *Typhoid present.*
(158)	" ..	"	5 "	" "	
(155)	Flask 1 R ..	"	3 "	Turbid in 24 hours.	
(159)	" ..	"	5 "	" 72 "	
				Uninfected Unsterilised Loch Katrine (Second Series).	
(152)	Flask 1 I ..	1·0	3 drops	Turbid in 48 hours.	Plate cultivations of the tubes Nos. 152 and 157 yielded somewhat expanding milk-drop colonies, and which were further differentiated from typhoid by giving bubbles in gelatine, and thick growths on potatoes, but no indol or milk reactions.
(156)	" ..	"	5 "	Did not become turbid.	
(153)	Flask 1 R ..	"	3 "	Turbid in 48 hours.	
(157)	" ..	"	5 "	" "	

			Typhoid-infected Unsterilised Loch Katrine (Second Series).		
21.7.1893					
(189)	Flask 1 I ..	1·0	3 drops	Turbid in 24 hours.	Plate cultivations of the tube No. 193 gave colonies which were recognised and confirmed as typhoid, besides other colonies resembling the *B. coli communis*, and answering to the same tests (gas bubbles, indol, milk, and potatoes). The plate cultivations of tube No. 194, on the other hand, gave only typhoid colonies which were duly confirmed. *Typhoid present.*
(193)	" ..	"	5 "	" 48 "	
(190)	Flask 1 R ..	"	3 "	" " "	
(194)	" ..	"	5 "	" " "	
			Uninfected Unsterilised Loch Katrine (Second Series).		
(187)	Flask 1 I ..	1·0	3 drops	Turbid in 72 hours.	
(191)	" ..	"	5 "	Only turbid in 8 days.	
(188)	Flask 1 R ..	"	3 "	Turbid in 72 hours.	
(192)	" ..	"	5 "	Only turbid in 8 days.	

Thus when the typhoid bacilli were introduced into the L. Katrine water in large numbers, they were still easily discoverable by phenol broth-culture on the fourteenth day, although from the examinations by plate-cultivation (see p. 488) it is obvious that their numbers had undergone enormous diminution. They doubtless persisted even longer than this, but the experiments had to be interrupted. Thus when introduced in large numbers their persistence is greater than when only small numbers are employed, for in the previous experiments they were no more demonstrable in the unsterilised water which had been kept at a summer temperature (19° C.) for 11 days (see p. 476).

Comparative Behaviour of the Typhoid Bacillus in Thames, Loch Katrine, and Deep Well Water.

The previous experiments had clearly shown that the typhoid bacillus, although unable to multiply in either ordinary Thames or L. Katrine water, even when these waters are deprived of other competing or inimical bacteria, is yet able to remain alive for considerable periods of time in these waters, not only when they are previously sterilised, but even, although for a distinctly shorter period, in their unsterilised condition and in the presence of an abundant bacterial population.

Inasmuch as the access of typhoid bacilli to potable water of all kinds is one of the most ever-present dangers to the public health, it becomes a matter of pressing hygienic importance to determine whether the particular kind of water into which they may gain access affects the chance of their reaching the water-consumer in a living state. The population of the United Kingdom is chiefly supplied with one or other of three different kinds of water, of which the Thames, L. Katrine, and deep-well water of the Kent Company may be taken as types, and it is with these three types of water that I have, therefore, instituted the comparison in question.

From the experiments which I have detailed above, it is obvious that the longevity of the typhoid bacillus in any particular water is subject to very considerable variations according to the initial vitality of the typhoid bacillus employed, and according as a relatively large or small number of the bacilli is introduced into the water. In order, therefore, to institute a comparison between several different waters as to their relative capacity of maintaining the typhoid bacilli in a living state, it is absolutely essential that the typhoid bacilli placed in the several waters should be taken from one and the same cultivation, and that they should be introduced in each case in as far as possible the same numbers.

These were the conditions which were secured in the series of comparative experiments made with these three different types of water, and which are now to be described.

Simultaneous Infection with Typhoid of Thames, Loch Katrine, and Deep Well Water.

Each of these waters was, in this comparative series, simultaneously experimented with in the natural unsterilised state, also after sterilisation by steam, as well as after sterilisation by filtration through a porous cylinder composed of baked infusorial earth. These nine different kinds of water were all infected at one time with the same quantity of typhoid bacilli taken from one and the same cultivation.

For this purpose an agar-cultivation of sixteen days' age, and grown at 18—20° C., was employed. Forty needle loops were carefully taken from the surface of this cultivation and thoroughly mixed by prolonged agitation with 50 c.c. of sterilised tap-water. Of the water-attenuation thus prepared 4 c.c. were added to 1000 c.c. of each of the nine different kinds of water. In this manner was, therefore, secured the equal infection both qualitatively and quantitatively of each of the nine experimental waters. Each of these infected waters was subdivided amongst several sterile flasks. The mouths of these flasks instead of being plugged with cotton-wool, were in this series of experiments simply covered with sterile beakers, an arrangement which is in many respects preferable for purposes of this kind. All these flasks, together with similar flasks containing each of the three unsterilised waters not infected, were placed in a dark cupboard in which there prevailed an almost uniform temperature of 9—12° C.

In this series of experiments, besides determining the relative longevity of the typhoid bacilli in the several different types of potable water, I have also endeavoured to ascertain the effect on the bacteria of keeping the waters at rest and in motion respectively. To this end, in the case of each water, one flask was kept at rest and only shaken up when a sample was to be taken from it, whilst the other flask was daily submitted to violent agitation over a period of five minutes, this agitation being repeated two or three times on the same day. The convention will be adopted in the following pages of referring to the flasks kept at rest by the letter A, whilst those which were subjected to daily agitation are distinguished by the letter B.

The various waters were periodically examined both by plate-cultivation and phenol-broth culture on the same lines as described for the previous series of experiments.

The uninfected waters yielded the following results on chemical analysis:—

It is in this way that particular interest attaches to a comparison of the results obtained by gelatine plate and phenol broth-culture in the case of these infected sterile waters, as the two methods can be made to control each other, whilst in the case of the infected unsterilised waters the method of phenol broth-culture has to be exclusively relied on for the detection of the typhoid and coli bacilli.

Behaviour of the Typhoid Bacillus in the Loch Katrine Water. (Second Series of Experiments.)

In the first series of experiments with the L. Katrine water recorded above, the number of typhoid bacilli initially introduced was so small that it would obviously not be possible to directly compare the results with those previously obtained with Thames water in which a much larger number of typhoid bacilli were initially introduced, as I have found in previous investigations of the same kind that one of the factors determining the longevity of pathogenic bacteria placed in water, or for the matter of that placed in any unfavourable surroundings, is the absolute number in which they are present. In other words, amongst, for instance, 1,000 bacteria taken from a given source there may be *some individuals* which will resist a particular adverse influence, whilst amongst 10 bacteria taken from the same source there may be *none* capable of resisting the adverse influence in question.

When, therefore, I found that such a small number of typhoid bacilli had been introduced into the L. Katrine water in the first series of experiments, I immediately started a second series of experiments with the same water, but introducing a much larger number of typhoid bacilli.

In this second series of Loch Katrine experiments, which were begun on 7.7.1893, or three days after the first, only unsterilised Loch Katrine water was infected with typhoid, thus :—

Infection of L. Katrine Water in Second Series of Experiments.—25 needle-loops were taken from the surface of an agar-culture of the typhoid bacillus, 11 days old, and introduced into 20 c.c. of steam-sterilised tap-water, which was then violently shaken for 15 minutes; 10 c.c. of this water-attenuation were then added to 1500 c.c. of unsterilised L. Katrine water. After thorough mixture this was divided up amongst a number of sterilised flasks plugged with sterile cotton-wool, which were placed in the incubator (19° C.) and refrigerator (6—8° C.) respectively. Control flasks containing the same unsterilised L. Katrine water, but uninfected, were placed under precisely similar conditions.

The results of bacteriological examination of the uninfected control L. Katrine water are given in the following table :—

Unsterilised Uninfected Loch Katrine Water. (Second Series.)

Dates on which plate cultivations were made.	Particular flask employed.		Number of days plates were incubated.	Volume of water employed for plate cultivation.	Number of colonies obtained from 1 c.c. of water.	
	Incubator.	Refrigerator.			Incubator flask.	Refrigerator flask.
7.7.1893	Before subdivision.		3	c.c. $\frac{2}{11}$ and $\frac{1}{11}$	410	
11.7.1893	1 I	1 R	3 3	$\frac{1}{5}, \frac{1}{25}, \frac{1}{50}, \frac{1}{60}$ $\frac{1}{5}, \frac{1}{10}, \frac{1}{25}, \frac{1}{50}$	4550	412
18.7.1893	1 I	1 R	6 3	$\frac{2}{5}$ and $\frac{1}{5}$ $\frac{1}{5}$ and $\frac{1}{10}$	39	335
21.7.1893	1 I	1 R	3 3—4	$\frac{2}{15}$ and $\frac{1}{15}$ $\frac{1}{5}$ and $\frac{1}{12}$	114	657

N.B.—Only a small number of liquefying colonies on any of the above plates.

Typhoid-infected Unsterilised Loch Katrine Water. (Second Series.)

Dates on which plate cultivations were made.	Particular flask employed.		Number of days plates were incubated.	Volume of water employed for plate cultivations.	Number of colonies obtained from 1 c.c. of water.	
	Incubator.	Refrigerator.			Incubator flask.	Refrigerator flask.
7.7.1893	Before subdivision.		3	c.c. $\frac{1}{30}$ and $\frac{1}{50}$	441,000	
11.7.1893	1 I		3	$\frac{1}{25}$ and $\frac{1}{50}$	298,000 (Plates badly liquefied, so that numbers somewhat uncertain, and doubtless under-estimated.)	
		1 R	3	$\frac{1}{25}$ and $\frac{1}{50}$		250,000 (Plates also badly liquefied. This liquefaction indicates that the water-bacteria must have undergone great multiplication.)
18.7.1893	1 I		4	$\frac{1}{50}$	8250 (A few surface colonies, doubtless typhoid.)	
		1 R	3	$\frac{1}{25}$ and $\frac{1}{50}$		253,000 (Very numerous surface colonies, doubtless typhoid.)
	In the plates from both waters a number of liquefying colonies present.					
21.7.1893	1 I		2	$\frac{1}{30}$ and $\frac{1}{50}$	3800 (Several expansion colonies, doubtless typhoid.)	
		1 R	3	$\frac{1}{25}$ and $\frac{1}{50}$		3000 (Several expansion colonies, doubtless typhoid.)
	The plates from both waters contained a number of liquefying colonies.					

Thus in these uninfected L. Katrine waters only comparatively slight multiplication took place, and at no time were there more than a few colonies causing liquefaction of the gelatine.

With these must now be compared the L. Katrine water infected with typhoid, the results obtained with which are recorded in the table (p. 488).

These examinations show that although the water-bacteria present in the unsterilised L. Katrine water must have undergone considerable multiplication, as shown by the great increase in the liquefying colonies, yet this multiplication did not by any means keep pace with the decrease in the number of typhoid bacilli; for the total number of colonies on the successive plates underwent continuous decline.

The actual proof of the persistence of the typhoid bacillus in these waters had of course to be furnished by the method of phenol broth-culture. Thus

Examination of Unsterilised Loch Katrine Waters (Second Series) by Phenol Broth-culture.

Number of broth tube.	Water used for cultivation with phenol broth.	Volume of water taken in c.c.	Quantity of phenol solution added to 10 c.c. broth.	Remarks.	
				Typhoid-infected Unsterilised Loch Katrine (Second Series).	
8.7.1893					
(124)	Flask 1 I ..	1·0	3 drops	Turbid in 48 hours.	On plate cultivation the tubes Nos. 133 and 134 yielded typical typhoid colonies, which were further confirmed by usual tests. *Typhoid present.*
(133)	" ..	"	5 "	" "	
(125)	Flask 1 R ..	"	3 "	" "	
(134)	" ..	"	5 "	" "	
15.7.1893					
(154)	Flask 1 I ..	"	3 "	Turbid in 72 hours.	On plate cultivation the tubes Nos. 155, 158 and 159, yielded typical typhoid colonies, which were further confirmed by usual tests. *Typhoid present.*
(158)	" ..	"	5 "	" "	
(155)	Flask 1 R ..	"	3 "	Turbid in 24 hours.	
(159)	" ..	"	5 "	" 72 "	
				Uninfected Unsterilised Loch Katrine (Second Series).	
(152)	Flask 1 I ..	1·0	3 drops	Turbid in 48 hours.	Plate cultivations of the tubes Nos. 152 and 157 yielded somewhat expanding milk-drop colonies, and which were further differentiated from typhoid by giving bubbles in gelatine, and thick growths on potatoes, but no indol or milk reactions.
(156)	" ..	"	5 "	Did not become turbid.	
(153)	Flask 1 R ..	"	3 "	Turbid in 48 hours.	
(157)	" ..	"	5 "	" "	

Typhoid-infected Unsterilised Loch Katrine (Second Series).

21.7.1893					
(189)	Flask 1 I ..	1·0	3 drops	Turbid in 24 hours.	Plate cultivations of the tube No. 193 gave colonies which were recognised and confirmed as typhoid, besides other colonies resembling the *B. coli communis*, and answering to the same tests (gas bubbles, indol, milk, and potatoes). The plate cultivations of tube No. 194, on the other hand, gave only typhoid colonies which were duly confirmed. *Typhoid present.*
(193)	,, ..	,,	5 ,,	,, 48 ,,	
(190)	Flask 1 R ..	,,	3 ,,	,, ,, ,,	
(194)	,, ..	,,	5 ,,	,, ,, ,,	

Uninfected Unsterilised Loch Katrine (Second Series).

(187)	Flask 1 I ..	1·0	3 drops	Turbid in 72 hours.
(191)	,, ..	,,	5 ,,	Only turbid in 8 days.
(188)	Flask 1 R ..	,,	3 ,,	Turbid in 72 hours.
(192)	,, ..	,,	5 ,,	Only turbid in 8 days.

From the above tables it will be seen that the water-bacteria in the deep well water underwent much more extensive multiplication than in either the Thames or Loch Katrine water. This extensive multiplication of the bacteria in such deep well water was first called attention to by me in 1886 ('Proc. Roy. Soc.'). The infected water must have contained, initially, about 28,000 typhoid bacilli per c.c., and in this infected water the increase in the total number of bacteria was even more marked than in the uninfected, the increase in the number of liquefying colonies being altogether enormous. The behaviour of the typhoid bacilli in the sterile deep well water (see p. 505) clearly shows that they cannot have participated in this bacterial multiplication observed in the infected unsterilised deep well water, but on the contrary they must have undergone great diminution, as *it will be shown on p.* 515 *that the typhoid bacilli were no longer discovered by phenol broth culture in this water after* 21.11.1893, *or thirty-three days after their first introduction.*

Comparison of Thames, Loch Katrine, and Deep Well Waters.

Typhoid-infected Steam-sterilised Thames Water.

Dates on which plate cultivations were made.	Particular flask employed.		Number of days plates were incubated.	Volume of water employed for plate cultivation.	Number of colonies obtained from 1 c.c. of water.	
	Kept at rest.	Daily agitated.			Flask kept at rest.	Flask daily agitated.
19.10.1893	Before subdivision		5	c.c. $\frac{1}{2}$ and $\frac{1}{5}$	10,980	
30.10.1893	A		7	$\frac{8}{11}$ and $\frac{3}{11}$	4,100	
		B	7	$\frac{2}{3}$ and $\frac{1}{6}$		4,700
8.11.1893	A		6	$\frac{8}{11}$ and $\frac{2}{11}$	2,400	
		B	6	$\frac{8}{11}$ and $\frac{2}{11}$		1,850
20.11.1893	A		9	$\frac{4}{5}$ and $\frac{1}{5}$	118	
27.11.1893	A		8	$\frac{2}{3}$ and $\frac{1}{6}$	0	

Typhoid-infected Porcelain-filtered Thames Water.

19.10.1893	Before subdivision		5	$\frac{1}{2}$ and $\frac{1}{5}$	9,800	
30.10.1893	A		8	$\frac{8}{11}$ and $\frac{2}{11}$	The plates contained no colonies resembling typhoid, but a very large number of small yellow colonies very similar to colonies found in the unfiltered plates. The organism giving rise to these colonies had probably passed through the filter in small numbers, and had then undergone enormous multiplication in the filtered water.	
		B	8	$\frac{2}{3}$ and $\frac{1}{6}$		

From the above tables it will be seen that *in the steam sterilised Thames water the typhoid bacilli underwent no multiplication, but, on the contrary, steady diminution, being last discovered on* 20.11.1893, *or thirty-two days after their first introduction.*

In the Thames water, sterilised by filtration, their disappearance was far more rapid, for they were no longer discoverable eleven days after their introduction, and they had doubtless died off even before this. These results entirely confirm my previous experiences recorded on p. 464, and the confirmation is of the more importance, as the filters used in the two cases were entirely different; thus whilst that used on the former occasion was a Chamberland cylinder of porous porcelain, the one used in this latter instance was a small porous cylinder constructed of infusorial earth. These infusorial earth cylinders are much more porous than the porcelain ones, and pass the water far more rapidly.

Comparison of Thames, Loch Katrine, and Deep Well Waters.

Typhoid-infected Steam-sterilised Loch Katrine Water.

Dates on which plate cultivations were made.	Particular flask employed.		Number of days plates were incubated.	Volume of water employed for plate cultivation.	Number of colonies obtained from 1 c.c. of water.	
	Kept at rest.	Daily agitated.			Flask kept at rest.	Flask daily agitated.
19.10.1893	Before subdivision		5	c.c. $\frac{5}{10}$ and $\frac{2}{10}$	12,000	
30.10.1893	A		7	$\frac{3}{5}$ and $\frac{1}{5}$	7,500	
		B	7	$\frac{5}{10}$ and $\frac{2}{10}$		7,200
8.11.1893	A		6	$\frac{4}{5}$ and $\frac{1}{5}$	3,800	
		B	6	$\frac{5}{10}$ and $\frac{2}{10}$		3,100
20.11.1893	A		7	$\frac{3}{5}$ and $\frac{1}{5}$	790	
27.11.1893	A		8	$\frac{3}{5}$ and $\frac{2}{5}$	220	
9.12.1893	A		9	$1\frac{1}{5}$ and $\frac{3}{5}$	250	

Typhoid-infected Porcelain-filtered Loch Katrine Water.

Dates on which plate cultivations were made.	Kept at rest.	Daily agitated.	Number of days plates were incubated.	Volume of water employed for plate cultivation.	Flask kept at rest.	Flask daily agitated.
19.10.1893	Before subdivision		5	$\frac{5}{15}$ and $\frac{1}{3}$	11,000	
30.10.1893	A		7	$\frac{5}{10}$ and $\frac{2}{10}$	9,000	
		B	7	$\frac{5}{10}$ and $\frac{2}{10}$		9,000
8.11.1893	A		6	$\frac{5}{15}$ and $\frac{2}{15}$	4,800	
		B	6	$\frac{5}{10}$ and $\frac{2}{10}$		4,100
20.11.1893	A		7	$\frac{5}{10}$ and $\frac{2}{10}$	2,300	
		B	7	$\frac{5}{10}$ and $\frac{2}{10}$		2,200
27.11.1893	A		8	$\frac{5}{15}$ and $\frac{2}{15}$	1,400	
9.12.1893	A		9	1·0 and 0·5	0	

The above tables show that *in the steam-sterilised Loch Katrine water, the typhoid bacilli again underwent no multiplication, but on the contrary steady decline, the last surviving individuals being, however, remarkably persistent. Thus the typhoid bacilli were still discoverable on* 9.12.1893, *or fifty-one days after their first introduction. This is a much longer survival than in the case either of the steam-sterilised Thames or deep well waters.*

In the Loch Katrine water, rendered sterile by filtration, the typhoid bacilli also survived much longer than in either the similarly treated Thames or deep well waters, the bacilli being still discovered on 27.11.1893, *or thirty-nine days after their first introduction.* The filter used in this case was a porous cylinder of infusorial earth as with the Thames and deep well waters. This result again confirms what I found in the previous series of experiments (see pp. 460, 464, 479), viz., that the typhoid bacilli persisted much longer in the porcelain-filtered Loch Katrine than in the porcelain-filtered Thames water.

Comparison of Thames, Loch Katrine, and Deep Well Waters.

Typhoid-infected Steam-sterilised Deep Well Water.

Dates on which plate cultivations were made.	Particular flask employed.		Number of days plates were incubated.	Volume of water employed for plate cultivation.	Number of colonies obtained from 1 c.c. of water.	
	Kept at rest.	Daily agitated.			Flask kept at rest.	Flask daily agitated.
19.10.1893	Before subdivision		5	c.c. $\frac{5}{11}$ and $\frac{2}{11}$	10,300	
30.10.1893	A		6	$\frac{8}{11}$ and $\frac{2}{11}$	2,900	
"		B	6	$\frac{8}{13}$ and $\frac{2}{13}$		2,800
8.11.1893	A		6	$\frac{2}{3}$ and $\frac{1}{6}$	1,100	
"		B	6	$\frac{8}{13}$ and $\frac{2}{13}$		840
20.11.1893	A		15	$\frac{4}{5}$ and $\frac{1}{5}$	0	

Typhoid-infected Porcelain-filtered Deep Well Water.

19.10.1893	Before subdivision		5	$\frac{1}{3}$ and $\frac{1}{5}$	12,200	
30.10.1893	A		13	$\frac{8}{11}$ and $\frac{2}{11}$	0	
"		B	13	$\frac{6}{11}$ and $\frac{2}{11}$		0
8.11.1893	A		12	3·0 and 2·0	0	
"		B	12	3·0 and 2·0		0

From the above tables it is seen that *in the steam-sterilised deep well water also the typhoid bacilli were incapable of multiplication, and, on the contrary, underwent continuous decline in numbers; they were last discovered on* 8.11.1893, *or twenty days after their introduction, whilst on* 20.11.1893, *or after being in the water for thirty-two days, they were no longer discoverable by plate-cultivation.* In this connection it is particularly noteworthy that the typhoid bacilli were still discovered on 21.11.1893 in the unsterilised deep well water, *thus showing that in this deep well water their longevity was unaffected by the circumstance of whether the water was sterilised or unsterilised.* In the case of both the Thames and Loch Katrine waters, on the other hand, the longevity of the typhoid bacilli was much greater in the sterilised than in the unsterilised water.

This circumstance is particularly instructive and important, inasmuch as it was just in this typhoid-infected unsterilised deep well water that the water bacteria present multiplied most extensively. and yet this large multiplication of the common water forms did not prejudicially affect the typhoid bacilli.

This deep well water, on the other hand, is in the sterilised condition less favourable to the longevity of the typhoid bacilli than the sterilised Thames and Loch Katrine waters, for in these three steam-sterilised waters the introduced typhoid bacilli disappeared first in the deep well and last in the Loch Katrine water, their longevity in the steam-sterilised Thames water being greater than in the deep well and less than in the Loch Katrine water. (For further remarks on this behaviour see p. 517.)

In the deep well water sterilised by filtration through porous porcelain (in this case again infusorial earth), the typhoid bacilli again disappeared with remarkable promptitude, being no longer discoverable eleven days after their introduction.

In order to ascertain whether these waters sterilised by filtration through porous cylinders owed the rapid disappearance of the typhoid and coli bacilli which almost invariably occurred in them to the presence of any antiseptic substance possessing general bactericidal properties, the following experiment was made:—

The typhoid-infected porcelain-filtered deep well water referred to above, and in which the typhoid bacillus was proved to be extinct on 30.10.1893, and 8.11.1893 respectively (see Table, p. 505), was on 11.11.1893 treated with three drops of the unsterilised uninfected deep well water. These three drops of unsterile water must have contained about 3000 water bacteria, as calculated from the results of plate cultivation given in the table on p. 499, and, as the volume of filtered water to which these three drops were added was about 100 c.c., the latter must have acquired about thirty water bacteria per 1 c.c. by the addition.

This porcelain-filtered deep well water, to which the three drops of unsterile deep well water were thus added on 11.11.1893, was examined by plate cultivation on 14.11.1893, *or three days after the addition, and was then found to contain* 10,462 *bacteria per* 1 *c.c.*; it was again examined *on* 23.11.1893, *or twelve days after the addition, and then contained* 603,900 *bacteria. It is obvious, therefore, that in this same water in which the typhoid bacilli were destroyed with such remarkable rapidity, some at any rate of the common water bacteria present in the unsterile deep well water were able to multiply both to an enormous extent and with wonderful celerity.* This result dismisses, in my opinion, the last lurking suspicion which might still remain of any antiseptic substance having accidentally gained access to the water in the process of filtration through these porous cylinders.

Comparison of Thames, Loch Katrine, and Deep Well Waters. Examination by Phenol Broth-culture

Date and number of broth tube.	Water used for cultivation with phenol broth.	Volume of water taken, c.c.	Quantity of phenol solution added to 10 c.c. broth.	Remarks.
20.10.1893	*Thames.*			
(1)	Uninfected unsterilised (A).....	1·0	3 drops	Turbid in 24 hours. On plate cultivation obtained a number of large surface milk-drop colonies and small depth colonies, often lenticular in shape. These yielded bubbles in gelatine, coagulated milk, gave a strong growth on potatoes, but a negative indol reaction. *Therefore neither typhoid nor* B. coli communis.
(10)	" " (A).....	"	5 "	Did not go turbid.
(4)	Typhoid-infected unsterilised (A)	"	3 "	Turbid in 24 hours. These were not submitted to plate cultivation, as there could be no doubt that *typhoid was present.*
(13)	" " (A)	"	5 "	Turbid in 48 hours.
(7)	Typhoid-infected steam sterilised (A)	4 drops	3 "	Turbid in 24 hours.
(16)	Typhoid-infected steam sterilised (A)	4 "	5 "	Turbid in 24 hours.
28.10.1893				
(19)	Uninfected unsterilised (A).....	1·0	3 drops	Turbid in 24 hours. Plate cultivations yielded colonies of various kinds, including liquefying ones, but none resembling typhoid.
(25)	" " (A).....	"	5 "	Turbid in 5 days.
(31)	" " (B).....	"	3 "	Turbid in 24 hours.
(37)	" " (B).....	"	5 "	Did not go turbid.
(22)	Typhoid-infected unsterilised (A)	"	3 "	Turbid in 24 hours. Plate cultivations yielded liquefying colonies (probably *B. liquidus*), but also numerous typical surface expansion colonies of typhoid bacillus. *Typhoid present.*
(28)	" " (A)	"	5 "	Did not go turbid.
(34)	" " (B)	"	3 "	Turbid in 24 hours. Plate cultivations yielded principally colonies causing liquefaction (probably *B. liquidus*), no colonies resembling typhoid. *Typhoid absent.*
(40)	" " (B)	"	5 "	Did not go turbid.

1.11.1893 (43)	Uninfected unsterilised (A).....	1·0	3 drops	Turbid in 48 hours. Plate cultures gave principally liquefying colonies (probably *B. liquidus*), nothing like typhoid.
(49)	" " (A).....	"	5 "	Did not go turbid.
(55)	" " (B).....	"	3 "	Turbid in 48 hours. Same results as with No. 43 above.
(61)	" " (B).....	"	5 "	Did not go turbid.
(46)	Typhoid-infected unsterilised (A)	"	3 "	Turbid in 24 hours. Plate cultures gave only liquefying colonies, nothing like typhoid. *Typhoid absent.*
(52)	" " (A)	"	5 "	Turbid in 72 hours. Plate cultures gave only very large thick milk-drop expansion colonies, giving bubbles in gelatine, but negative indol reaction. *Typhoid absent.*
(58)	" " (B)	"	3 "	Turbid in 24 hours Plate cultures gave only liquefying colonies and milk-drop colonies, nothing like typhoid. *Typhoid absent.*
(64)	" " (B)	"	5 "	Turbid in 72 hours. Plate cultures gave only milk-drop colonies, with faint resemblance to typhoid, but yielded bubbles in gelatine, negative indol, however. *Typhoid absent.*
7.11.1893 (76)	Uninfected unsterilised (A).....	1·0	3 drops	Turbid in 48 hours.
(82)	" " (A).....	"	5 "	Turbid in 48 hours. Plate cultures contained characteristic colonies with thickened rim and clearer centre, not like typhoid.
(88)	" " (B).....	"	3 "	Turbid in 48 hours. Plate cultures contained only liquefying colonies (probably *B. liquidus*).
(94)	" " (B).....	"	5 "	Did not go turbid.
(79)	Typhoid-infected unsterilised (A)	"	3 "	Turbid in 24 hours. Plate cultures contained only liquefying colonies (probably *B. liquidus*). *Typhoid absent.*
(85)	" " (A)	"	5 "	Did not go turbid.
(91)	" " (B)	"	3 "	Turbid in 24 hours. Results similar to those with No. 79. *Typhoid absent.*
(97)	" " (B)	"	5 "	Turbid in 48 hours. Plate cultures exhibited some surface colonies something like typhoid, but yellower; these gave bubbles in gelatine, but negative indol reaction. *Typhoid absent.*

Comparison of Thames, Loch Katrine, and Deep Well Waters. Examination by Phenol Broth-culture.

Date and number of broth tube.	Water used for cultivation with phenol broth.	Volume of water taken, c.c.	Quantity of phenol solution added to 10 c c. broth.	Remarks.
20.10.1893	*Loch Katrine.*			
(3)	Uninfected unsterilised (A).....	1·0	3 drops.	Turbid in 24 hours.
(12)	" " (A).....	"	5 "	Did not go turbid.
(6)	Typhoid-infected unsterilised (A)	"	3 "	Turbid in 24 hours.
(15)	" " (A)	"	5 "	Turbid in 24 hours.
(9)	Typhoid-infected steam sterilised (A)	"	3 "	Turbid in 24 hours. No plate cultures were made, as there could be no doubt as to presence of typhoid. *Typhoid present.*
(18)	Typhoid-infected steam sterilised (A)	"	5 "	Turbid in 24 hours.
28.10.1893 (21)	Uninfected unsterilised (A).....	1·0	3 drops	Turbid in 24 hours. Plates contained only liquefying colonies (probably *B. liquidus*), nothing resembling typhoid.
(27)	" " (A).....	"	5 "	Did not go turbid.
(33)	" " (B).....	"	3 "	Turbid in 72 hours.
(39)	" " (B).....	"	5 "	Did not go turbid.
(24)	Typhoid-infected unsterilised (A)	"	3 "	Turbid in 24 hours. Plates yielded the typical surface expansion colonies, which were proved to be typhoid by the usual tests. *Typhoid present.*
(30)	" " (A)	"	5 "	Did not go turbid.
(36)	" " (B)	"	3 "	Turbid in 24 hours.
(42)	" " (B)	"	5 "	Turbid in 24 hours. Plates exhibited a pure cultivation of typhoid. *Typhoid present.*

1.11.1893				
(45)	Uninfected unsterilised (A).....	1·0	3 drops	Did not go turbid.
(51)	" " (A).....	"	5 "	
(57)	" " (B).....	"	3 "	
(63)	" " (B).....	"	5 "	
(48)	Typhoid-infected unsterilised (A)	"	3 "	Turbid in 48 hours. Plates yielded pure cultivation of typhoid, confirmed by usual tests. *Typhoid present.*
(54)	" " (A)	"	5 "	Did not go turbid.
(60)	" " (B)	"	3 "	Turbid in 48 hours.
(66)	" " (B)	"	5 "	Turbid in 48 hours. Plates yielded pure cultivation of typhoid, confirmed by usual tests. *Typhoid present.*
7.11.1893				
(78)	Uninfected unsterilised (A).....	1·0	3 drops	Did not become turbid.
(84)	" " (A).....	"	5 "	
(90)	" " (B).....	"	3 "	
(96)	" " (B).....	"	5 "	
(81)	Typhoid-infected unsterilised (A)	"	3 "	Turbid in 48 hours.
(87)	" " (A)	"	5 "	Turbid in 48 hours. Plates gave a pure cultivation of typhoid, confirmed by usual tests. *Typhoid present.*
(93)	" " (B)	"	3 "	Turbid in 24 hours. Plates gave a pure cultivation of typhoid, confirmed by usual tests. *Typhoid present.*
(99)	" " (B)	"	5 "	Did not become turbid.
21.11.1893				
(126)	Uninfected unsterilised (A).....	1·0	3 drops	Did not become turbid.
(130)	" " (A).....	"	5 "	
(127)	Typhoid-infected unsterilised (A)	"	3 "	Turbid in 48 hours. Plates gave pink liquefying colonies (very like *B. prodigiosus*), nothing like typhoid. *Typhoid absent.*
(131)	" " (A)	"	5 "	Did not become turbid.
27.11.1893				
(160)	Uninfected unsterilised (A).....	1·0	3 drops	Turbid in 48 hours. Plates completely liquefied.
(164)	" " (A).....	"	5 "	Did not become turbid.
(161)	Typhoid-infected unsterilised (A)	"	3 "	Turbid in 48 hours. Plates contained pink liquefying colonies (like *B. prodigiosus*), nothing like typhoid on plate. *Typhoid absent.*
(165)	" " (A)	"	5 "	Did not become turbid.

Comparison of Thames, Loch Katrine, and Deep Well Waters. Examination by Phenol Broth-culture—*continued.*

Date and number of broth tube.	Water used for cultivation with phenol broth.	Volume of water taken, c.c.	Quantity of phenol solution added to 10 c.c. broth.	Remarks.
4.12.1893	*Loch Katrine*—cont.			
(184)	Uninfected unsterilised (A).....	1·0	3 drops	Not turbid in five days. *Typhoid absent.*
(195)	" " (A).....	"	5 "	
(185)	Typhoid-infected unsterilised (A)	"	3 "	
(196)	" " (A)	"	5 "	
7.12.1893 (206)	Typhoid-infected unsterilised (A)	6·0	3 "	Turbid in 48 hours. Plates contained pink liquefying colonies as above, no surface colonies like typhoid; some depth colonies which gave bubbles in gelatine, but no indol reaction. *Typhoid absent.*

Comparison of Thames, Loch Katrine, and Deep Well Waters. Examination by Phenol Broth-culture.

Date and number of broth tube.	Water used for cultivation with phenol broth.	Volume of water taken in c.c.	Quantity of phenol solution added to 10 c.c. broth.	Remarks.
20.10.1893	*Deep Well Water* (*Kent*).			
(2)	Uninfected unsterilised (A).....	1·0	3 drops	Turbid in 48 hours. Plates contained pinhead and small milk-drop colonies, which gave bubbles in gelatine, coagulated milk, but yielded no indol.
(11)	„ „ (A).....	„	5 „	Did not become turbid.
(5)	Typhoid-infected unsterilised (A)	„	3 „	Turbid in 24 hours. No plate cultures made from these, as of course at this stage *typhoid present.*
(14)	„ „ (A)	„	5 „	„ „
(8)	Typhoid-infected steam-sterilised (A)	4 drops	3 „	
(17)	Typhoid-infected steam-sterilised (A)	„	5 „	Turbid in 24 hours.
28.10.1893				
(20)	Uninfected unsterilised (A).....	1·0	3 drops	Turbid in 72 hours.
(26)	„ „ (A).....	„	5 „	Did not become turbid.
(32)	„ „ (B).....	„	3 „	Turbid in 48 hours. Plates contained small smooth-rimmed depth colonies giving rise to very small milk-drop expansions not like typhoid.
(38)	„ „ (B).....	„	5 „	Did not become turbid.
(23)	Typhoid-infected unsterilised (A)	„	3 „	Turbid in 24 hours. The majority of the colonies on the plates were liquefying (probably *B. liquidus*), but also a number of small colonies which may be typhoid.
(29)	„ „ (A)	„	5 „	Turbid in 72 hours. Plates contained large number of milk-drop colonies, but also some typical typhoid colonies. *Typhoid present.*
(35)	„ „ (B)	„	3 „	Turbid in 24 hours.
(41)	„ „ (B)	„	5 „	Turbid in 24 hours. These plates also gave a mixture of milk-drop and typical typhoid colonies, the latter were confirmed by usual tests. *Typhoid present.*

Comparison of Thames, Loch Katrine, and Deep Well Waters. Examination by Phenol Broth-culture—*continued.*

Date and number of broth tube.	Water used for cultivation with phenol broth.	Volume of water taken in c.c.	Quantity of phenol solution added to 10 c.c. broth.	Remarks.
	Deep Well Water (*Kent*)—cont.			
1.11.1893 (44)	Uninfected unsterilised (A).....	1·0	3 drops	Turbid in 72 hours. Plates contained only colonies liquefying gelatine (apparently *B. liquidus* and *B. fluorescens liquefaciens*).
(50)	" " (A).....	"	5 "	Did not become turbid.
(56)	" " (B).....	"	3 "	Turbid in 72 hours. Plates contained only liquefying colonies (apparently *B. liquidus*).
(62)	" " (B).....	"	5 "	Did not become turbid.
(47)	Typhoid-infected unsterilised (A)	"	3 "	Turbid in 24 hours. Plates contained well-defined typhoid colonies besides numerous liquefying colonies. *Typhoid present.*
(53)	" " (A)	"	5 "	Turbid in 48 hours. Plates contained principally large milk-drop colonies, but a few typhoid colonies also discernible, and these confirmed by usual tests. *Typhoid present.*
(59)	" " (B)	"	3 "	Turbid in 24 hours.
(65)	" " (B)	"	5 "	Turbid in 24 hours. Plates again contained principally large milk-drop colonies, but also some typical typhoid colonies, which were confirmed by usual tests. *Typhoid present.*
				The organisms giving rise to these milk-drop colonies appear to grow more readily in the phenol broth than the typhoid bacillus, hence the latter was more easily discoverable in the tubes which had only received 3 drops of phenol.
7.11.1893 (77)	Uninfected unsterilised (A).....	1·0	3 drops	Did not become turbid.
(83)	" " (A).....	"	5 "	Did not become turbid.
(89)	" " (B).....	"	3 "	Turbid in 48 hours. Plates contained fluorescent liquefying colonies, nothing like typhoid.
(95)	" " (B).....	"	5 "	Did not become turbid.
(80)	Typhoid-infected unsterilised (A)	"	3 "	Turbid in 24 hours. Plates contained both milk-drop and typical typhoid colonies, the latter confirmed by usual tests. *Typhoid present.*
(86)	" " (A)	"	5 "	Turbid in 48 hours. Plates contained only the milk-drop colonies.
(92)	" " (B)	"	3 "	Turbid in 24 hours. Plates contained both liquefying, milk-drop, and typical typhoid colonies, the latter confirmed by usual tests. *Typhoid present.*
(98)	" " (B)	"	5 "	Turbid in 48 hours. Plates contained both milk-drop and typical typhoid colonies. *Typhoid present.*

21.11.1893				
(124)	Uninfected unsterilised (A).....	1·0	3 drops	Turbid in 48 hours. Plates contained almost only fluorescent liquefying colonies. Nothing resembling typhoid.
(128)	" " (A).....	"	5 "	Did not become turbid.
(125)	Typhoid-infected unsterilised (A)	"	3 "	Turbid in 24 hours. Plates contained, besides liquefying colonies, also a very large number of milk-drop colonies, but none resembling typhoid.
(129)	" " (A)	"	5 "	Turbid in 48 hours. Plates contained a large number of thick surface colonies which gave bubbles in gelatine, but no indol in broth. Also one typical typhoid colony which was confirmed by usual tests. *Typhoid present.*
27.11.1893				
(158)	Uninfected unsterilised (A)	1·0	3 drops }	Did not become turbid.
(162)	" " (A).....	"	5 " }	
(159)	Typhoid-infected unsterilised (A)	"	3 "	Turbid in 24 hours.
(163)	" " (A)	"	5 "	Turbid in 48 hours. The plates contained, besides liquefying colonies, also some surface expansion colonies somewhat like typhoid but rather too thick, and these all gave bubbles in gelatine, although no indol reaction in broth. *Typhoid absent.*
4.12.1893				
(182)	Uninfected unsterilised (A).....	1·0	3 drops }	Did not become turbid.
(193)	" " (A).....	"	5 " }	
(183)	Typhoid-infected unsterilised (A)	"	3 "	Turbid in 24 hours. Plates contained an apparently pure culture of the organism producing thick milk-drop colonies, and giving as usual bubbles in gelatine, but no indol reaction. *Typhoid absent.*
(194)	" " (A)	"	5 "	Turbid in 48 hours. These plates again contained only the thick milk-drop colonies. *Typhoid absent.*

The above three tables are of especial importance, exhibiting as they do the different behaviour of the typhoid bacillus *taken from one and the same cultivation and in approximately the same numbers*, on being introduced into these three different kinds of water in their natural unsterilised condition.

From the first table it will be seen that *in the Thames water the typhoid bacilli were demonstrable on* 28.10.1893, i.e., *nine days after their introduction, but already four days later, and afterwards, all endeavours to discover the typhoid bacillus proved abortive.*

From the second table, on the other hand, it will be seen that *in the Loch Katrine water the presence of the typhoid bacillus was demonstrable on* 7.11.1893, i.e., *nineteen days after its introduction, whilst on* 21.11.1893, *or fourteen days later, and afterwards, it could no more be discovered.*

From the third table, again, it will be seen that *in the deep-well water the typhoid bacilli were easily discoverable on* 7.11.1893, i.e., *nineteen days after their introduction, and just discoverable on* 21.11.1893, *or thirty-three days after their introduction, whilst, six days later and thereafter, all attempts to demonstrate their presence proved fruitless.*

As regards the effect of agitation in these experiments, it appears that the agitation on the whole promoted the multiplication of the water bacteria in the unsterilised waters, whilst it somewhat accelerated the disappearance of the typhoid bacilli in the infected sterile waters. These results partially confirm those obtained by Professor Ray Lankester in some similar experiments described by him to the recent Royal Commission on the London Water Supply (Appendix, p. 455).

In his experiments 2 litres of sterilised river water were placed in two similar jars, and each was similarly infected with the typhoid bacillus. One of the jars was then syringed four times an hour for twelve hours, and, after an interval, for eight hours more. The other jar was left undisturbed in the dark. The syringed jars showed a very marked inferiority in the number of typhoid germs obtained on cultivation, amounting to a reduction of one-half.

In my experiments the difference between the waters kept at rest and those submitted to agitation was not nearly so marked, which may possibly be due to the different mode of agitation employed, and also to the fact that the waters were not examined so soon after the agitation, for it is quite possible that the combined effect of agitation and oxygenation makes itself felt more in the first instance than later on.

In the case of the infected unsterilised waters, again, there is considerable evidence that the agitation hastened the disappearance of the typhoid bacilli. *Thus in the case of the unsterile Thames water,*

the typhoid bacilli were discovered on 28.10.1893, *or eight days after infection in the flask which was kept at rest, whilst they were not demonstrable on the same day in the case of the flask which had been subjected to agitation.*

In the unsterile Loch Katrine water, again, although on 7.11.1893 typhoid bacilli were found both in the water which had been kept at rest and in that which had been agitated, yet the broth-tube with five drops of the phenol solution only became turbid in the case of the water which had remained at rest, thus tending to show that the typhoid bacilli were more numerous or in a more active condition in this latter water than in that which had been agitated.

From the experiments made with the sterilised Thames, Katrine, and deep well waters, it is evident that in none of these waters does the typhoid bacillus proliferate, the longevity of the bacilli being greatest in the Katrine and least in the deep-well water; on the other hand, in the unsterilised waters their longevity is decidedly greatest in the deep-well, and decidedly least in the Thames, water.

In the sterilised water the principal factor determining longevity would, therefore, appear to be the proportion of organic matter present, which is much greater in the Loch Katrine and Thames than in the deep well water; on the other hand, in the unsterilised water the factor determining longevity must be an entirely different one; and whatever it may be, it is more conspicuous in the case of the two surface (Thames and Loch Katrine) than in that of the subterranean (deep well) water. It is, as has already been frequently indicated, generally believed that the more rapid disappearance of pathogenic bacteria in unsterile than in sterile waters is due to the multiplication or competition of the common water bacteria in the unsterile waters; but these experiments clearly show that this cannot be true, at any rate without some qualification, for by reference to the tables on pp. 495, 497, and 499, it will be seen that it was precisely in the case of the deep well water that the most extensive multiplication of the water bacteria took place. In this connection it is interesting to compare the following statement made by Professor Ray Lankester, F.R.S., to the recent Royal Commission on the London Water Supply (Appendix, p. 458) :—

"I took pure cultures of a very active and common fluviatile form, *B. fluorescens liquefaciens*, which suggested, by its vigorous action on gelatine, the possession of destructive properties. With this I mixed a pure culture of *Bacillus typhosus*, and studied the mixed culture, both by drop culture under the microscope and in the tube. During a fortnight no diminution of the activity or numbers of either species was observed. I have experimented with pure cultures of other fluviatile species, and intend to continue the observations. It is possible—indeed not improbable—that one or more species may be

discovered which are injurious to, or destructive of, *B. typhosus*, but I have not yet succeeded in establishing the fact."

It naturally suggests itself that possibly the particular kinds of water bacteria present in the three different kinds of water experimented with may have influenced the relative longevity of the typhoid bacillus in these three waters respectively. With a view to putting this supposition to the test, the following series of experiments were made:—

(*a.*) One portion of the typhoid-infected steam-sterilised Thames water was inoculated with a few drops of unsterilised Thames water, a second portion with a few drops of unsterilised Loch Katrine water, and a third portion with a few drops of unsterilised deep well water.

(*b.*) One portion of the typhoid-infected steam-sterilised Loch Katrine water was inoculated with a few drops of unsterilised Thames water, a second portion with a few drops of unsterilised Loch Katrine water, and a third portion with a few drops of unsterilised deep well water.

(*c.*) One portion of the typhoid-infected steam-sterilised deep well water was inoculated with a few drops of unsterilised Thames water, a second portion with a few drops of unsterilised Loch Katrine water, and a third portion with a few drops of unsterilised deep well water.

Unfortunately, this series of experiments was only commenced on 16.11.1893, when the number of typhoid bacilli in the three steam-sterilised waters had got very low (see Tables, pp. 501, 503, 505). In all instances there was an enormous multiplication of the water bacteria introduced in the few drops of unsterilised water added in each case, but owing to the small number of typhoid bacilli present, it was not possible to establish whether the rate of their disappearance was differently affected by the multiplication of the water bacteria from the unsterile Thames, Loch Katrine, or deep well water respectively.

Owing to the failure of the above series of experiments to determine whether the undoubtedly greater bactericidal properties of the unsterilised over the sterilised waters are due to the multiplication of the contained water bacteria, or to some other cause, the following fresh series of experiments was conducted on Thames water alone with the object of elucidating the same point.

Experiments on the Relative Longevity of the Typhoid Bacillus in Unsterilised Thames Water and in Steam-sterilised Thames Water Reinoculated with Thames Water Bacteria. (11.1.1894.)

In this series of experiments one portion of a sample of Thames water collected at Hampton on January 9th, 1894, was infected with typhoid in the natural unsterile condition, a second portion was similarly infected after having been previously sterilised by steaming, and a portion of the latter was again supplied with the Thames water bacteria by inoculating it with a few drops of the same unsterilised Thames water.

Infection of the Waters.—I have found in the previous series of experiments that, in taking the surface-growth from a well-matured typhoid culture, a large proportion of the bacilli are dead, whilst even those which are alive are often in a more or less weakened state. On this account I now generally prefer to take the bacilli for experiments such as these directly from fresh plate cultivations, selecting the largest and most vigorous looking colonies.

In the present series of experiments a gelatine plate culture (five days old) of the typhoid bacillus was employed; fifty-seven surface colonies were carefully removed by means of a sterile platinum loop and transferred to 50 c.c. of steam-sterilised Thames water in a small sterile-stoppered bottle; this was then violently shaken for fifteen minutes as usual, measured quantities of this liquid being then used for the infection of the large volumes of water. Thus, 800 c.c. of steam-sterilised Thames water were infected with 8 c.c. of the above liquid, 400 c.c. of this was kept, and will be referred to in the following experiments as

"*Typhoid-infected Steam-sterilised Thames Water.*"

To the remaining 400 c.c. was added 1 c.c. of the unsterilised Thames water, with the object of imparting to it the several kinds of water bacteria in Thames water. This portion will be referred to in the following experiments as

"*Typhoid-infected Steam-sterilised Thames Water inoculated with a few drops of Unsterile Thames Water.*"

Again, 400 c.c. of unsterilised Thames water were inoculated with 4 c.c. of the water attenuation of typhoid mentioned above, and this will be referred to in the following pages as

"*Typhoid-infected Unsterilised Thames Water.*"

These several infected waters, as well as some of the uninfected unsterilised Thames water, were placed in sterile flasks covered with

sterile beakers, and kept in a dark cupboard at a temperature of 9—11° C. All of these waters were periodically submitted to plate cultivation, as well as tested for typhoid bacilli by the method of phenol broth-culture. The results of these examinations are recorded in the following tables :—

Uninfected Unsterilised Thames Water (16.1.1894).

Dates on which plate cultivations were made.	Number of days plates were incubated.	Volume of water employed for plate cultivation.	Number of colonies obtained from 1 c.c. of water.	Remarks.
16.1.1894	4	c.c. $\frac{1}{50}$ and $\frac{1}{100}$	5250	Only a very small number of liquefying colonies.
22.1.1894	8	$\frac{1}{100}$	5000	Ditto.
29.1.1894	8	$\frac{1}{50}$ and $\frac{1}{100}$	3750	Ditto.
5.2.1894	4	$\frac{1}{50}$ and $\frac{1}{120}$	2825	Ditto.
12.2.1894	7	$\frac{1}{50}$ and $\frac{1}{100}$	5500	No liquefying colonies.
19.2.1894	5	$\frac{1}{50}$ and $\frac{1}{150}$	3375	Ditto.
24.2.1894	5	$\frac{1}{50}$ and $\frac{1}{100}$	3750	Very few liquefying colonies only.

The above table shows that the sample of Thames water employed contained a considerable total number of bacteria, but of these unusually few caused liquefaction of the gelatine. Moreover, the water was characterised by the number of bacteria remaining practically stationary during the period (five weeks) over which these experiments extended, a phenomenon which I have already pointed out is not unfrequently met with in the case of surface waters.

Typhoid-infected Unsterilised Thames Water (16.1.1894).

Dates on which plate cultivations were made.	Number of days plates were incubated.	Volume of water employed for plate cultivation.	Number of colonies obtained from 1 c.c. of water.	Remarks.
16.1.1894	4	c.c. $\frac{1}{60}$ and $\frac{1}{120}$	175,000	The plates contained only very few liquefying colonies, and an immense number of small colonies (typhoid, of course).
22.1.1894	4	$\frac{1}{60}$ and $\frac{1}{120}$	58,000	No liquefying colonies.
29.1.1894	4	$\frac{1}{60}$ and $\frac{1}{120}$	36,000	Some liquefying colonies, but also a number of typical typhoid surface expansion colonies, and a number of small depth colonies which may also be typhoid.
5.2.1894	4	$\frac{1}{50}$ and $\frac{1}{100}$	29,000	A few liquefying colonies, numerous small depth colonies, but no typical surface expansion typhoid colonies.
12.2.1894	5	$\frac{1}{120}$	6,000	Ditto.
19.2.1894	4	$\frac{1}{50}$ and $\frac{1}{100}$	6,300	Ditto.
24.2.1894	5	$\frac{1}{50}$ and $\frac{1}{100}$	6,700	Ditto.

Bearing in mind (see table, p. 520) that the uninfected unsterilised Thames water contained about 5000 water bacteria in 1 c.c. on the day of infection, it is evident that about 170,000 typhoid bacilli per c.c. were introduced. The periodical examinations recorded above show that these numbers steadily declined, and from the character of the colonies obtained on the plates, it appears that the water bacteria underwent no marked multiplication, thus, no increase in the number of liquefying colonies was observed. The gradual diminution in the total number of colonies obtained may be ascribed, therefore, to the dying off of the 170,000 typhoid bacilli per c.c. introduced (see also results of phenol broth-culture experiments, p. 525, *et seq.*).

Typhoid-infected Steam-sterilised Thames Water (16.1.1894).

Dates on which plate cultivations were made.	Number of days plates were incubated.	Volume of water employed for plate cultivation.	Number of colonies obtained from 1 c.c. of water.	Remarks.
16.1.1894	4	c.c. $\frac{3}{5}$ and $\frac{2}{5}$	103,000	Pure cultivation of typhoid.
22.1.1894	6	$\frac{1}{5}$ and $\frac{1}{10}$	129,000	Ditto. The increase in number is so slight that it is not attributable to multiplication but rather to the breaking up of aggregates, and possibly also to the longer incubation of the plates.
29.1.1894	..	..	..	Plates accidentally lost.
5.2.1894	5	$\frac{2}{11}$ and $\frac{1}{11}$	76,500	Pure cultivation of typhoid, but the surface colonies have only very slightly expanded owing to the plate being much crowded.
12.2.1894	3	$\frac{1}{6}$ and $\frac{1}{12}$	64,600	Ditto.
19.2.1894	5	$\frac{1}{5}$ and $\frac{1}{10}$	17,080	Ditto.
24.2.1894	5	$\frac{2}{5}$ and $\frac{1}{5}$	5,337	Ditto.

As usual, then, in the steam-sterilised water, the typhoid bacilli underwent a gradual decline, the slight increase on 22.1.1894 being, in my opinion, not attributable to real multiplication, but to other causes as indicated above. The typhoid bacilli were still abundantly present thirty-nine days after their first introduction. I attribute this greater longevity in this series of experiments partly to the fact that the typhoid bacilli were initially introduced in such large numbers, and partly to their doubtless being in a very vigorous condition, having been taken from colonies selected for their size, and only five days old at the time.

Typhoid-infected Steam-sterilised Thames Water inoculated with a few drops of Unsterile Thames Water (16.1.1894).

Dates on which plate cultivations were made.	Number of days plates were incubated.	Volume of water employed for plate cultivation.	Number of colonies obtained from 1 c.c. of water.	Remarks.
16.1.1894	4	c.c. $\frac{1}{5}$ and $\frac{1}{5}$	115,000	Practically pure cultivation of typhoid.
22.1.1894	3	$\frac{1}{5}$ and $\frac{1}{10}$	116,500	Very large number of fluorescent liquefying colonies, also a large number of surface expansion colonies which may be typhoid, and a large number of small depth colonies.
29.1.1894	3	$\frac{1}{50}$ and $\frac{1}{100}$	44,875	Numerous liquefying colonies, numerous small depth colonies, only a few typical surface expansion colonies resembling typhoid. On the same day the presence of typhoid was *far* more conspicuous on the plates of the typhoid-infected unsterilised water (see Table, p. 521).
5.2.1894	4	$\frac{1}{100}$	7,200	Ditto.
12.2.1894	4	$\frac{1}{100}$	5,200	Ditto.
19.2.1894	3	$\frac{1}{50}$ and $\frac{1}{100}$	2,350	Numerous liquefying colonies, and depth colonies, but no typical surface expansions like typhoid, although of course the depth colonies *may* be typhoid.
24.2.1894	4	$\frac{1}{50}$ and $\frac{1}{100}$	36,600	Ditto.
7.3.1894	5	$\frac{1}{100}$	18,500	Ditto.

The 115,000 bacteria per 1 c.c. contained in this water must have been almost exclusively typhoid bacilli, for, after infection with typhoid, 400 c.c. were inoculated with 1 c.c. of unsterile Thames water. Now, this unsterile water, as seen from table, p. 520, contained about 5000 water bacteria per 1 c.c.; 5000 water bacteria must thus have been added to 400 c.c. of this typhoid-infected steam sterilised Thames water, which would thus contain about twelve water bacteria per 1 c.c. in addition to the typhoid bacilli with which it had been previously infected. These few water bacteria must have undergone rapid and extensive multiplication, for, on 22.1.1894, a

very large number of fluorescent liquefying colonies was found on the plates, and in all the subsequent examinations of this water numerous liquefying colonies were also present, thus clearly showing that the diminution in the total number of colonies found in the successive plates must have been in large measure due to the dying off of the typhoid bacilli.

Thus, whilst in the case of the simply typhoid-infected unsterilised Thames water there is no evidence (see table, p. 521) of multiplication of the contained water bacteria having taken place, in this typhoid-infected steam-sterilised Thames water, to which a few water bacteria had been added, there is evidence of abundant multiplication of these water bacteria having occurred, and it becomes a matter of the greatest importance to ascertain in which of these two waters the typhoid bacilli (present in each case in approximately the same numbers and having exactly the same origin and history) exhibited the greater longevity. This question was determined by means of the examinations by phenol broth-culture, the results of which are recorded in the following tables:—

Thames Water (series of Experiments begun 16.1.1894). Examinations by Phenol Broth-culture.

Date and number of broth tube.	Water used for cultivation with phenol broth.	Volume of water taken in c.c.	Quantity of phenol solution added to 10 c.c. broth.	Remarks.
22.1.1894.	*Thames Water.*			
(304)	Unsterilised uninfected	1·0	3 drops	Turbid in 24 hours. Plate cultivations exhibited a pure cultivation of what was apparently *B. liquidus* (Percy Frankland), *nothing resembling typhoid colonies on the plates.*
(308)	Ditto	"	5 "	Did not become turbid.
(305)	Typhoid-infected unsterilised	"	3 "	Turbid in 24 hours.
(309)	Ditto	"	5 "	Turbid in 24 hours. Plates gave typhoid surface expansion and depth colonies, confirmed by negative milk, negative indol, potatoes, and negative bubbles as usual. *Typhoid present.*
(302)	Typhoid-infected steam-sterilised	"	3 "	Turbid in 24 hours. } No plates poured, as, of course, *typhoid present.*
(306)	Ditto	"	5 "	Turbid in 24 hours. }
(303)	Typhoid-infected steam-sterilised inoculated with few drops of unsterile Thames	"	3 "	Turbid in 24 hours.
(307)	Ditto	"	5 "	Turbid in 24 hours. Plates gave typical typhoid colonies, confirmed by usual tests. *Typhoid present.*
29.1.1894.				
(314)	Unsterilised uninfected	1·0	3 drops	Turbid in 48 hours. Plates exhibited a pure cultivation of what was apparently *B. liquidus, nothing resembling typhoid colonies on the plates.*
(318)	Ditto	"	5 "	Did not become turbid.
(315)	Typhoid-infected unsterilised	"	3 "	Turbid in 24 hours. } Plates from both gave typical typhoid colonies, confirmed by usual tests. *Typhoid present.*
(319)	Ditto	"	5 "	Turbid in 24 hours. }
(312)	Typhoid-infected steam-sterilised	"	3 "	Turbid in 24 hours. Plates gave typical typhoid colonies, confirmed by usual tests. *Typhoid present.*
(316)	Ditto	"	5 "	Turbid in 24 hours.
(313)	Typhoid-infected steam-sterilised inoculated with few drops of unsterile Thames	"	3 "	Turbid in 24 hours. Plates gave typical typhoid colonies, confirmed by usual tests. *Typhoid present.*
(317)	Ditto	"	5 "	Turbid in 48 hours.

Thames Water (series of Experiments begun 16.1.1894). Examinations by Phenol Broth culture—*continued.*

Date and number of broth tube.	Water used for cultivation with phenol broth.	Volume of water taken in c.c.	Quantity of phenol solution added to 10 c.c. broth.	Remarks.
5.2.1894.	*Thames Water*—cont.			
(322)	Unsterilised uninfected.	1·0	3 drops	Turbid in 48 hours. Plates again exhibited a pure cultivation of what was apparently *B. liquidus*, *nothing resembling typhoid colonies on the plates.*
(326)	Ditto	,,	5 ,,	Did not become turbid.
(323)	Typhoid-infected unsterilised	,,	3 ,,	Turbid in 24 hours. } Plates from both gave typical typhoid colonies, confirmed by usual tests. *Typhoid present.*
(327)	Ditto	,,	5 ,,	Turbid in 48 hours.
(320)	Typhoid-infected steam-sterilised	,,	3 ,,	Turbid in 24 hours. } Plates from both gave typical typhoid colonies, confirmed by usual tests. *Typhoid present.*
(324)	Ditto	,,	5 ,,	Turbid in 24 hours.
(321)	Typhoid-infected steam-sterilised inoculated with few drops of unsterile Thames.	,,	3 ,,	Turbid in 24 hours. } Plates from both gave typical typhoid colonies, confirmed by usual tests. *Typhoid present.*
(325)	Ditto	,,	5 ,,	Turbid in 48 hours.
12.2.1894.				
(332)	Unsterilised uninfected.	1·0	3 drops	} Not turbid after 5 days.
(336)	Ditto	,,	5 ,,	
(333)	Typhoid-infected unsterilised.	,,	3 ,,	} Not turbid after 5 days. *Typhoid absent.*
(337)	Ditto	,,	5 ,,	
(330)	Typhoid-infected steam-sterilised.	,,	3 ,,	Turbid in 24 hours. } No plates poured, but, of course, *typhoid present.*
(334)	Ditto	,,	5 ,,	Turbid in 48 hours.
(331)	Typhoid-infected steam-sterilised inoculated with few drops of unsterile Thames.	,,	3 ,,	Turbid in 24 hours. } Plates from both gave typical typhoid colonies, confirmed by usual tests. *Typhoid present.*
(335)	Ditto	,,	5 ,,	Turbid in 72 hours.

15.2.1894.				
(350)	Uninfected unsterilised	1·0	3 drops	Did not become turbid.
(352)	Ditto	3·0	„	Turbid in 48 hours. Plates gave pure cultivation of what appeared to be *B. liquidus*. No colonies like typhoid.
(351)	Typhoid-infected unsterilised	1·0	„	Turbid in 48 hours. Plates gave pure cultivation of what appeared to be *B. liquidus*. No colonies like typhoid. *Typhoid absent.*
(353)	Ditto	3·0	„	Turbid in 48 hours. Plates gave large number of small colonies not unlike typhoid. On inoculation into gelatine, found to liquefy on long keeping. On potatoes gave diffused, colourless growth rather more conspicuous than typhoid, but still not sufficiently conclusive. Poured gelatine plates from potato culture, crowded plates had the gelatine softened, and the colonies which had not liquefied on the less crowded ones caused liquefaction on inoculating into gelatine and keeping. *Typhoid absent.*
19.2.1894.				
(360)	Unsterilised uninfected	1·0	3 drops	Did not become turbid.
(361)	Ditto	3·0	„	Turbid in 48 hours. Plates contained colonies of *B. liquidus* and small smooth-rimmed colonies; the latter, on inoculation into broth, rendered the latter turbid and formed a pellicle on the surface, therefore not typhoid.
(362)	Typhoid-infected unsterilised	1·0	„	Did not become turbid.
(363)	Ditto	3·0	„	Turbid in 48 hours. Plates gave a pure cultivation of what appeared to be *B. liquidus*, and contained nothing like typhoid colonies. *Typhoid absent.*
(358)	Typhoid-infected steam-sterilised	1·0	„	Turbid in 24 hours. No plates poured, but, of course, *typhoid present.*
(359)	Typhoid-infected steam-sterilised inoculated with few drops of unsterile Thames	„	„	Turbid in 48 hours. Plates exhibited typical typhoid depth colonies, but many of the depth colonies were curiously lobulated, and had even corkscrew-like prolongations; fresh plates were poured from one of these depth colonies, and these contained only typical typhoid colonies, both depth and surface. *Typhoid present.*
26.2.1894.				
(380)	Typhoid-infected steam-sterilised	1·0	3 drops	Turbid in 48 hours. No plates poured, but, of course, *typhoid present.*
(381)	Typhoid-infected steam-sterilised inoculated with few drops of unsterile Thames	„	„	Did not become turbid. *Typhoid absent.*
1.3.1894.				
(384)	Typhoid-infected steam-sterilised	1·0	3 drops	Turbid in 24 hours. Plates gave typical typhoid colonies, confirmed by usual tests. *Typhoid present.*
(386)	Typhoid-infected steam-sterilised inoculated with few drops of unsterile Thames	„	„	Did not become turbid in 4 days.
(385)	Ditto	3·0	„	Turbid in 4 days. The plates gave a pure cultivation of what appeared to be *B. liquidus*, and contained nothing like typhoid colonies. *Typhoid absent.*
5.3.1894.				
(387)	Typhoid-infected steam-sterilised	1·0	3 drops	Turbid in 48 hours. *Typhoid present.*
(388)	Ditto	3·0	„	

The above table clearly shows

(1.) That *the uninfected unsterilised Thames water* contained no bacteria which could be mistaken for typhoid bacilli by the methods of investigation employed. The phenol broth-tubes which became turbid on inoculation with this water were always found by plate cultivation to yield colonies which have all the appearance of the *B. liquidus* formerly described by me ('Zeitschrift für Hygiene,' vol. 6, 1889; 'Roy. Soc. Proc.,' vol. 53, 1893, p. 186). In all these experiments in which the method of phenol broth-culture has been employed, I have repeatedly found that this same organism succeeds in developing in the broth to which three drops of the phenol solution per 10 c.c. of broth are added.

(2.) That *the typhoid-infected steam-sterilised Thames water* contained living typhoid bacilli throughout the entire period of forty-eight days (January 16th—March 5th, 1894) over which this series of experiments extended.

(3.) That in *the typhoid-infected unsterilised Thames water*, the typhoid bacilli were still demonstrable on 5.2.1894, or twenty days after their introduction; but seven days later, on 12.2.1894, they were no longer discoverable.

(4.) That in *the typhoid-infected steam-sterilised Thames water, which had been furnished with the Thames water bacteria by inoculating with a few drops of unsterilised Thames water*, the typhoid bacilli were still demonstrable on 19.2.1894, or thirty-four days after their introduction, whilst seven days later, on 26.2.1894, they were no longer discoverable.

(5.) Thus, not only was the longevity of the typhoid bacilli far greater, as usual, in the sterilised than in the unsterile waters, but of the two unsterile waters, the one naturally so, and the other rendered unsterile by the inoculation of a few drops of unsterile Thames water, the naturally unsterile one proved to be decidedly more antagonistic to the vitality of the typhoid bacillus, than the water rendered artificially unsterile, as we may call it, by inoculation with unsterile Thames water.

(6.) This result is the more significant and important, inasmuch as it was shown (see p. 521) that in the naturally unsterile water no multiplication of the water bacteria took place, whilst, in what we may call the artificially unsterile water, it was shown (see p. 523) that a very large multiplication of the introduced water bacteria certainly did take place.

(7.) In the previous series of experiments (see p. 516) it was equally clearly shown that the typhoid bacilli enjoyed a greater longevity in the unsterile deep well water than in

either the unsterile Thames or Loch Katrine waters, although it was precisely in the deep well water that the water bacteria underwent multiplication.

(8.) These experimental observations lead me to the conclusion that the antagonistic action of the unsterile waters on the typhoid bacillus is not to be attributed to the multiplication of the water bacteria leading to the suppression of the typhoid bacilli "by competition in the struggle for existence," to use the common phraseology of many writers on these subjects, but through the existence in the unsterile waters of conditions (due, doubtless, to a great extent to the presence of chemical products elaborated by water bacteria) which are inimical to the vitality of the typhoid bacillus.

That conditions inimical to the vitality of the typhoid bacillus *can be generated by the water bacteria alone* is demonstrated by the above experiments in which a few drops of unsterile Thames water were added to the typhoid-infected steam-sterilised water, with the result that the longevity of the typhoid bacilli in this water was far less than in the steam-sterilised water itself.

That the longevity of the typhoid bacillus is still less in the naturally unsterile water than in that rendered unsterile by inoculation, I attribute to the fact that in the naturally unsterile Thames water, countless generations of water bacteria have flourished before the water is made the subject of experiment at all, and it must, therefore, be more or less saturated with those bacterial products which are prejudicial to the vitality of the typhoid bacillus, and which, in fact, frequently hamper or even inhibit the further multiplication of the water bacteria themselves.

The deep well water, on the other hand, in its natural condition is in a very different state; as my numerous former examinations (see Second Report, pp. 178—180) of this water have shown, it is drawn from its subterranean source in an almost perfectly sterile condition, having never since its exhaustive filtration through the porous strata of the earth, in which it has altogether altered its chemical composition, harboured any micro-organisms at all, so that the abundant bacterial multiplication, which, as I have shown, it exhibits in the laboratory, is really the first time that it is subjected to the influence of bacterial growth, and it takes a correspondingly longer time, therefore, for the conditions inimical to the typhoid bacillus to be established. In fact, in the experiments with deep well water (see pp. 505, 515) the typhoid bacillus was actually discovered in the unsterile water a little later than in the steam-sterilised water. That the multiplication of the bacteria in the deep well water does not lead to conditions so antagonistic to the vitality of the typhoid bacillus is also, no doubt, attri-

butable to the circumstance that the number of different kinds of bacteria in the deep well water is much more limited than in ordinary surface waters.

For the practical hygienic application of these experimental observations, reference should be made to the final conclusions at the end of this Report, see p. 543.

Further Experiments on the Influence of the Addition of Common Salt to Water containing the Typhoid Bacillus.

The several waters prepared for the last series of experiments were also made to serve the purpose of verifying the remarkable results previously obtained (see p. 434, *et seq.*) by the addition of common salt to typhoid-infected waters.

These experiments were commenced on 12.2.1894, on which day some of each of the following waters (for full particulars concerning them, see p. 519) received 1 per cent. of pure sterile sodium chloride respectively:—

(*a.*) *Typhoid-infected steam-sterilised Thames water.*
(*b.*) *Typhoid-infected steam-sterilised Thames water, inoculated with a few drops of unsterile Thames water.*
(*c.*) *Uninfected unsterilised Thames water.*
(*d.*) *Typhoid-infected unsterilised Thames water.*

These waters, to each of which 1 per cent. of sodium chloride had been added, were preserved in a dark cupboard at a temperature of 9—11° C., and were submitted to periodical examination along with the several waters of the last series, with which they were to be compared.

Uninfected Unsterilised Thames Water to which 1 per cent. of Sodium Chloride was added on 12.2.1894.

Dates on which plate cultivations were made.	Number of days plates were incubated.	Volume of water employed for plate cultivation.	Number of colonies obtained from 1 c.c. of water.	Remarks.
14.2.1894	5	c.c. $\frac{1}{50}$ and $\frac{1}{100}$	2,150	Only very small number of liquefying colonies.
19.2.1894	3	$\frac{1}{50}$ and $\frac{1}{100}$	607,000	Very large number of liquefying colonies.
24.2.1894	3	$\frac{1}{50}$ and $\frac{1}{100}$	1,200,000	Numerous liquefying colonies.

On comparing this table with the one on p. 520, which refers to the same water, only without the addition of salt, it will be seen that the effect of the salt addition was to cause an enormous and rapid multiplication of the water bacteria. The liquefying colonies underwent great multiplication, but there was also an enormous increase in other non-liquefying colonies of various kinds.

Typhoid-infected Unsterilised Thames Water to which 1 per cent. of Sodium Chloride was added on 12.2.1894.

Dates on which plate cultivations were made.	Number of days plates were incubated.	Volume of water employed for plate cultivation.	Number of colonies obtained from 1 c.c. of water.	Remarks.
14.2.1894	5	c.c. $\frac{1}{100}$	5,000	Only a very small number of liquefying colonies.
19.2.1894	8	$\frac{1}{50}$ and $\frac{1}{50}$	404,000	Considerable number of liquefying colonies, but special increase in small non-liquefying colonies.
24.2.1894	8	$\frac{1}{50}$ and $\frac{1}{100}$	407,000	Comparatively few liquefying colonies, but an enormous number of small non-liquefying colonies.

On comparing the above table with that on p. 521, it will be seen what an enormous multiplication of the bacteria present was caused by the addition of the salt; in this case the multiplication was not so much shared in by the liquefying organisms as by the others.

Typhoid-infected Steam-sterilised Thames Water to which 1 per cent. of Sodium Chloride was added on 12.2.1894.

Dates on which plate cultivations were made.	Number of days plates were incubated.	Volume of water employed for plate cultivation.	Number of colonies obtained from 1 c.c. of water.	Remarks.
14.2.1894	5	c.c. $\frac{1}{5}$ and $\frac{1}{10}$	17,385	Typical pure cultivation of typhoid.
19.2.1894	8	$\frac{1}{5}$ and $\frac{1}{10}$	360	Ditto.
24.2.1894	16	$\frac{5}{5}$ and $\frac{5}{15}$	6	Only depth colonies on the plate.

On comparing this table with that on p. 522, it will be seen that the addition of the salt was highly prejudicial to the typhoid bacilli, the latter exhibiting a most rapid diminution in number, and degeneration in their vitality.

Typhoid-infected Steam-sterilised Thames Water inoculated with a few drops of Unsterile Thames Water, to this 1 per cent. of Sodium Chloride was added on 12.2.1894.

Dates on which plate cultivations were made.	Number of days plates were incubated.	Volume of water employed for plate cultivation.	Number of colonies obtained from 1 c.c. of water.	Remarks.
14.2.1894	3	c.c. $\frac{1}{50}$ and $\frac{1}{100}$	25,725	Numerous liquefying colonies and large number of small depth colonies.
19.2.1894	2	$\frac{1}{50}$ and $\frac{1}{150}$	296,500	Ditto.
24.2.1894	3	$\frac{1}{50}$ and $\frac{1}{100}$	340,000	Ditto.

On referring to the table on p. 523, it will be seen that in this water, before the addition of the salt, a considerable multiplication of the water bacteria had taken place, but at the time the salt was added, the total number of bacteria only amounted to from 2000—5000 per 1 c.c.; after the addition of the salt, however, an enormous multiplication followed.

We must now see how the vitality of the typhoid bacilli was affected by this enormous bacterial multiplication which took place in the unsterile waters after the 1 per cent. of common salt was added to them. This was, of course, ascertained by the method of phenol broth-culture, the results of which are recorded in the following table:—

Thames Water with additions of 1 per cent. Common Salt (12.2.1894). Examinations by Phenol Broth-culture.

Date and number of broth tube.	Water used for cultivation with phenol broth.	Volume of water taken in c.c.	Quantity of phenol solution added to 10 c.c. broth.	Remarks.
14.2.1894	*Thames Water.*			
(340)	Uninfected unsterilised, 1 per cent. sodium chloride added, 12.2.1890	1·0	3 drops	Turbid in 48 hours. Plates gave a pure cultivation of what appeared to be *B. liquidus* (Percy Frankland). Nothing like typhoid colonies on plates.
(344)	Ditto	„	5 „	Not turbid in 5 days.
(341)	Typhoid-infected unsterilised, 1 per cent. sodium chloride added, 12.2 1894	„	3 „	Turbid in 48 hours. Plates gave a few liquefying and many depth colonies, some of which formed small expansions, not unlike typhoid. These found to coagulate milk, and to give greenish-yellow growth on potatoes, also liquefied gelatine slowly. *Not typhoid.*
(345)	Ditto	„	5 „	Not turbid in 5 days.
(338)	Typhoid-infected steam-sterilised, 1 per cent. sodium chloride added, 12.2.1894	„	3 „	Turbid in 24 hours. Plates gave typical typhoid colonies, confirmed by usual tests. *Typhoid present.*
(342)	Ditto	„	5 „	Turbid in 48 hours.
(339)	Typhoid-infected steam-sterilised, inoculated with few drops of unsterile Thames, 1 per cent. sodium chloride added, 12.2.1894	„	8 „	Turbid in 48 hours. Plates gave typical typhoid colonies, confirmed by usual tests. *Typhoid present.*
(343)	Ditto	„	5 „	Not turbid in 5 days.
19.2.1894				
(366)	Uninfected unsterilised, 1 per cent. sodium chloride added, 12.2.1894	1·0	8 drops	Did not become turbid.
(367)	Typhoid-infected unsterilised, 1 per cent. sodium chloride added, 12.2.1894	„	8 „	Did not become turbid. *Typhoid absent.*
(364)	Typhoid-infected steam-sterilised. 1 per cent. sodium chloride added, 12.2.1894	„	8 „	Turbid in 48 hours. Plates gave numerous depth colonies, but only one typical typhoid expansion. Many of the depth colonies were much lobulated, and exhibited whip-like prolongations. Fresh plates were poured from one of these colonies, and these plates exhibited the typical typhoid colonies. *Typhoid present.*
(365)	Typhoid-infected steam-sterilised, inoculated with few drops of unsterile Thames, 1 per cent. sodium chloride added, 12.2.1894	„	8 „	Did not become turbid. *Typhoid absent.*

Thames Water with additions of 1 per cent. Common Salt (12.2.1894). Examinations by Phenol Broth-culture—*continued.*

Date and number of broth tube.	Water used for cultivation with phenol broth.	Volume of water taken in c.c.	Quantity of phenol solution added to 10 c.c. broth.	Remarks.
	Thames Water—cont.			
22.2.1894 (369)	Uninfected unsterilised, 1 per cent. sodium chloride added, 12.2.1894	3·0	3 drops	Turbid in 4 days. Plates gave a pure cultivation of what appeared to be *B. liquidus*. Nothing like typhoid colonies on the plates.
(370)	Typhoid-infected unsterilised, 1 per cent. sodium chloride added, 12.2.1894	„	3 „	Turbid in 24 hours. Plates gave a pure cultivation of what appeared to be *B. liquidus*. Nothing like typhoid colonies on the plates. *Typhoid absent.*
(368)	Typhoid-infected steam-sterilised, inoculated with few drops of unsterile Thames, 1 per cent. sodium chloride added, 12.2.1894	„	3 „	Turbid in 48 hours. Plates gave only liquefying colonies. *Typhoid absent.*
26.2.1894 (382)	Typhoid-infected steam-sterilised, 1 per cent. sodium chloride added, 12.2.1894	1·0	3 drops	Turbid in 5 days. Plates gave the same lobulated colonies noticed above (Broth tube No. 364). *Typhoid present.*
(383)	Typhoid-infected steam-sterilised, inoculated with few drops unsterile Thames, 1 per cent. sodium chloride added, 12.2.1894	3·0	3 „	Turbid in 48 hours. Plates gave only liquefying colonies. *Typhoid absent*
5.3.1894 (389)	Typhoid-infected steam-sterilised, 1 per cent. sodium chloride added, 12.2.1894	1·0	3 drops	Not turbid in 7 days. } *Typhoid absent.*
(390)	Ditto	3·0	8 „	Not turbid in 7 days. }

The above table should be compared with that on p. 525 *et seq.*, which refers to the same waters only without the addition of salt. It will then be seen :—

(1.) That *in the typhoid-infected unsterilised Thames water*, the typhoid bacilli had already disappeared on 12.2.1894, *i.e.*, before the addition of salt was made at all, and, therefore, that no inferences can be drawn as to the effect of the salt on the typhoid bacilli in this unsterilised water. Of course the absence of typhoid in this unsterile water was not ascertained until more than a week after this date, 12.2.1894, on which the salt was added, otherwise the experiment would not have been made at all.

(2.) *In the typhoid-infected steam-sterilised Thames water, inoculated with a few drops of unsterile Thames*, the typhoid bacilli were still demonstrable on 19.2.1894, but not on 26.2.1894, whilst in the same water, to which 1 per cent. of salt was added on 12.2.1894, the typhoid bacilli were discoverable still on 14.2.1894, *but not on 19.2.1894.* Thus the addition of the salt considerably hastened the disappearance of the typhoid bacilli from what may be called this artificially unsterile Thames water.

(3.) As already shown by the results of plate cultivation given on p. 531, the addition of the 1 per cent. of salt to the *typhoid-infected steam-sterilised Thames water*, caused a very rapid diminution in the number of typhoid bacilli present, and these results are entirely confirmed by the results on cultivation with phenol broth. Thus, the last plate cultivation, made on 24.2.1894, revealed the presence of only six typhoid bacilli in 1 c.c., phenol broth-culture still showed the presence of typhoid on 26.2.1894, but no more on 5.3.1894, although in the same water, which had not been treated with sodium chloride, the typhoid bacilli were easily discoverable by phenol broth-culture on that day.

(4.) There can be no doubt, therefore, that common salt, whilst enormously stimulating the multiplication of many forms of water bacteria, exerts a directly and highly prejudicial effect on the typhoid bacilli, causing their rapid disappearance from the water whether water bacteria are present or not.

(5.) It is worthy of remark that the typhoid colonies obtained on plate cultivation of the phenol-broth tubes which had been rendered turbid by these infected saline waters exhibited in some cases a very abnormal appearance, being irregularly swollen and lobulated in the depth and often giving rise to whip-like prolongations into the surrounding gelatine. As

far as I am aware, such abnormally formed typhoid colonies have not been previously observed, and I am inclined to attribute them to the degeneration of the typhoid bacilli, for, on passing such colonies through a further process of plate cultivation, only the normally formed typhoid colonies were obtained.

On the Possibility of the Typhoid Bacillus or the B. coli communis *multiplying in Potable Water.*

In none of the several series of experiments with Thames, Loch Katrine, and deep well (chalk) water, already recorded, was there any evidence of the typhoid bacillus undergoing any multiplication whatsoever; on the contrary, in all cases in which the typhoid bacillus was introduced into these waters in a sterilised condition there was a more or less rapid diminution in its numbers observed. In the case of the *B. coli communis*, in the steam-sterilised Thames water (p. 454), there was considerable multiplication of the *B. coli communis* observed when the water was kept at a summer, but practically none when it was maintained at the winter, temperature; on the other hand, in the steam-sterilised Loch Katrine water (see p. 481) the *B. coli communis* did not exhibit any numerical increase, but, on the contrary, rapid decline.

Of the waters experimented with above, the Thames water contains about the average quantity of organic matter present in those surface waters from cultivated land which are supplied for domestic purposes, whilst similarly the extremely small proportion of organic matter in the Kent well water is typical of the water supplied from deep wells; on the other hand, the Loch Katrine water contains decidedly less organic matter than is usually present in the water supplied to towns from upland surface sources. Now, although the experiments which have been detailed above conclusively show that the typhoid bacillus does not proliferate in the Thames, deep well, or Loch Katrine water employed, it appeared to me to be of importance to ascertain whether in upland surface water, more highly impregnated with organic matter than is the case with that from Loch Katrine, such proliferation of the typhoid bacillus might not perhaps take place.

For this purpose I employed, as specially fitted for the object in view, some moorland surface water which is supplied to a large manufacturing population in North Britain, and which the following analysis shows is very highly charged with organic matter of a peaty character:—

	Results of analysis expressed in parts per 100,000.	
Total solid matter	8·80	The sample was turbid, yellow-brown in colour, possessed a peaty taste, and contained animalcules visible to the naked eye.
Organic carbon (by combustion)	0·623	
„ nitrogen („)	0·049	
„ „ (by Kjeldahl process)	0·044	
Ammonia (free)	0·0	
„ (albuminoid)	0·023	
Oxygen consumed by organic matter	0·405	
Nitrogen as nitrates and nitrites	0·015	
Total combined nitrogen	0·064	
Chlorine	0·8	
Temporary hardness	0·0	
Permanent „	3·1	
Total „	3·1	

One part of this peaty water was sterilised by steaming, and another part by filtration through a porous cylinder (Chamberland filter), and each of these sterilised waters was divided into two parts, which were respectively infected with the typhoid bacillus and with the *B. coli communis* on 30.10.1893, thus:—

Agar cultivations of the typhoid bacillus and of the *B. coli communis*, each twenty-seven days old and grown at 18—20° C., were employed:—

Twenty needle-loops of the typhoid growth were thoroughly shaken up with 25 c.c. of sterilised water.

Ten needle-loops of the growth of the *B. coli communis* were similarly shaken up with another 25 c.c. of sterilised water.

The peaty water was infected with the typhoid bacillus by adding 3 c.c. of the above water attenuation to 300 c.c. of the sterilised peaty water, whilst for infection with the *B. coli communis* only 1 c.c. of the water attenuation was added to 300 c.c. of the sterilised peaty water. The infected waters were placed in flasks plugged with cotton wool, and preserved in a dark cupboard at 10—12° C.

The fate of the bacilli (typhoid and coli) in this infected peaty water was then ascertained by means of periodical plate cultivations, which yielded the following results:—

Steam-sterilised Peaty Water, infected 30.10.1893 with—

Typhoid bacillus.	Plate cultivations made	*B. coli communis.*
1776 colonies from 1 c.c. (plates incubated 6 days).	30.10.1893	13,321 colonies from 1 c.c. (plates incubated 4—6 days).

Steam-sterilised Peaty Water, infected 30.10.1893 with—*continued.*

Typhoid bacillus.	Plate cultivations made	*B. coli communis.*
	6.11.1893	
1044 colonies from 1 c.c. (plates incubated 7 days).		14,081 colonies from 1 c.c. (plates incubated 7 days).
	15.11.1893	
432 colonies from 1 c.c. (plates incubated 5 days).		17,580 colonies from 1 c.c. (plates incubated 5 days).
	23.11.1893	
220 colonies from 1 c.c. (plates incubated 6 days).		14,457 colonies from 1 c.c. (plates incubated 4 days).

Peaty Water Sterilised by Filtration, infected 30.10.1893, with—

Typhoid bacillus.	Plate cultivations made	*B. coli communis.*
	30.10.1893	
1169 colonies from 1 c.c. (plates incubated 6 days).		21,603 colonies from 1 c.c. (plates incubated 4—6 days).
	6.11.1893	
No colonies on plates (plates incubated 9 days).		No colonies on plates (plates incubated 9 days).

Thus even in this water, heavily charged as it was with vegetable organic matter, the typhoid bacilli failed to undergo any multiplication, but, on the contrary, as usual, suffered a continuous numerical decline. The B. coli communis, *on the other hand, remained practically unaltered in numbers during the period of upwards of three weeks over which these observations extended, the slight numerical increase being too insignificant to be regarded as evidence of true multiplication.*

In the peaty water which had been sterilised by filtration through porous porcelain there was again the same phenomenon, so frequently referred to before in this Report, of the extraordinarily rapid disappearance of the introduced typhoid and coli bacilli.

On the possible Adaptation of the Typhoid Bacillus to active life in Potable Water.

In none of the experiments recorded above was any multiplication of the typhoid bacillus observed, although these experiments have been made with waters varying from the deep well water of the Kent Company, which is almost wholly destitute of organic matter, to the peaty water referred to on p. 537, which contains about the maximum amount of organic matter met with in water used for drinking purposes.

Some previous observers, on the other hand, record the multiplication of the typhoid bacillus in potable waters with which they have made similar experiments; whilst others, again, have found no multiplication. It appears to me highly probable that in most, if not in all, cases in which multiplication has been observed, it has been occasioned through the introduction of an appreciable amount of food-material along with the typhoid bacilli; for, as already pointed out, most investigators have exercised very little care in respect of this highly important factor.

But although the typhoid bacilli taken directly from an ordinary cultivation and plunged into potable water may not be able to proliferate in the latter, it appeared to me quite possible that if the environment of the typhoid bacilli were gradually, instead of suddenly, changed, the requirements of the bacilli might perhaps be thereby so far modified as to undergo multiplication in the aqueous medium. For by gradually changing the surroundings, it would be anticipated that those individuals most capable of flourishing under the altered conditions would be propagated, and that each successive generation of typhoid bacilli would thus become more adapted to the new medium.

To ascertain whether this process of education could be actually accomplished, the following experiments were made.

Education of Typhoid Bacilli for Aquatic Life.

A gelatine-culture of the typhoid bacillus was, in the first instance, inoculated into sterile broth of the ordinary strength, and kept at 18—20° C.; turbidity ensued in twenty-four hours. From this broth cultivation an inoculation was made into 50 per cent. broth (broth mixed with its own volume of water); this also became turbid in twenty-four hours at 18—20° C. From the 50 per cent. broth cultivation an inoculation was made into 10 per cent. broth (1 volume of broth mixed with 9 volumes of water); this liquid only became visibly turbid in from two and a half to three days. After four successive generations of cultivation in this 10 per cent. broth medium had been carried on, the time elapsing between the inoculation and the appearance of turbidity gradually diminished until, with the fifth generation, turbidity already set in in twenty-four hours. From this 10 per cent. broth, inoculation was then made into 1 per cent. broth (1 volume of broth mixed with 99 volumes of water); this became turbid in twenty-four hours. Continuous cultivation in this 1 per cent. broth medium was then carried on for a period of two months, after which it was employed for infecting steam-sterilised Thames water, thus:—

On 31.1.1894, 2 drops of a 1 per cent. broth cultivation (three

days old) of the typhoid bacillus were added to 600 c.c. of steam-sterilised Thames water, which was, on the day of infection and on several subsequent occasions, submitted to plate cultivation with the following results:—

Dates on which plates were prepared.	Number of days plates were incubated.	Number of typhoid bacilli in 1 c.c. of water.
31.1.1894	8	4,895
2.2.1894	7	15,372
5.2.1894	10	11,184
12.2.1894	7	6,558
23.2.1894	7	5,795
6.3.1894	6	6,068
10.3.1894	9	4,093

These figures show that unquestionable, although not very extensive, multiplication of the typhoid bacilli took place in the water thus infected; but in order to ascertain whether this proliferation was effected at the expense of the very small quantity of culture-material necessarily introduced along with the bacilli, or at the expense of the organic matter pertaining to the Thames water itself, the following further experiment was made:—

On 23.2.1894, 10 c.c. of the above-infected water, which on that day contained 5795 typhoid bacilli per 1 c.c., were added to 20 c.c. steam-sterilised Thames water, and the latter was then and several times subsequently submitted to plate cultivation with the following results:—

Dates on which plates were prepared.	Number of days plates were incubated.	Number of typhoid bacilli in 1 c.c. of water.
23.2.1894	7	1830
26.2.1894	7	1842
2.3.1894	6	375
6.3.1894	6	268

From these figures it is equally evident that in this case no multiplication but only numerical decline of the typhoid bacilli took place. If, however, the typhoid bacilli can proliferate at the expense of the organic matter belonging to the Thames water, they should have multiplied in the above experiment, as they were imported into a quantity of water, the organic matter of which had not since sterilisation been exposed to bacterial life; but from the fact that they did not multiply, but, on the contrary, only fell off in numbers, it becomes almost certain that the distinct multiplication observed in the former experiment was effected at the expense of the small quantity of food-material originally introduced into the water along with the typhoid bacilli.

Another experiment was made on similar lines to the first half of the above experiment, to see whether the multiplication there observed could be confirmed, thus:—

On 27.3.1894, 4 c.c. of a 1 per cent. broth cultivation (three days old) of the typhoid bacillus were put into 50 c.c. of steam-sterilised Thames water, the mixture being violently shaken up for fifteen minutes; 2 c.c. of this mixture (equivalent to $\frac{1}{27}$ c.c. of the original 1 per cent. broth culture) were then added to 200 c.c. of steam-sterilised Thames water, which was then submitted to plate cultivation as follows:—

Dates on which plates were prepared.	Number of days plates were incubated.	Number of typhoid bacilli in 1 c.c. of water.
27.3.1894	6	37,515
29.3.1894	6	61,566
31.3.1894	9	50,935
4.4.1894	6	27,818
11.4.1894	8	20,130

In this case again, therefore, there was a small but distinct multiplication.

From these experiments it appears that typhoid bacilli which have undergone a prolonged and gradual training in more and more aqueous culture-media do exhibit distinctly more vitality in potable water than bacilli which are at once transferred into water from highly nutritive solid media like agar or peptone jelly. On the other hand, there is considerable reason for believing that the slight but distinct multiplication which these trained bacilli undergo in potable water, is effected at the expense of small quantities of food-material introduced along with them at the time of infection, and not at the expense of the organic matter belonging to the water itself.

The result of the experiments with these specially-trained typhoid bacilli greatly fortifies the opinion which I have expressed above, that the extensive multiplication of typhoid bacilli in potable waters which has been observed by some investigators was most probably due to the importation of appreciable quantities of food-material along with the bacilli themselves.

Summary.

The investigation which has been detailed in the foregoing pages is divisible into the following sections:—

1. A series of experiments in which the vitality of one and the same culture of the typhoid bacillus was observed in one and the same sample of Thames water, using the latter under the following conditions:—

(*a*) In its natural unsterile state;
(*b*) Sterilised by steam;
(*c*) Sterilised by filtration through porous porcelain.

In each case the effect of temperature was studied by preserving the infected waters at winter and summer temperatures respectively.

(Description of experiments, see pp. 409—433, 451—465; summary of conclusions, pp. 433, 434.)

2. A perfectly similar series of experiments, carried on side by side with the above, in which the *Bacillus coli communis* was employed instead of the typhoid bacillus.

(Description of experiments, see pp. 409—433; summary of conclusions, pp. 433, 434.)

3. A series of experiments in which the effect of the addition of common salt in various proportions to unsterile Thames water was studied, the unsterile Thames water being employed for this purpose both uninfected and infected with the typhoid bacillus.

(Description of experiments, see pp. 434—450; summary of conclusions, p. 450.)

4. A series of experiments perfectly similar to No. 1 above, in which Loch Katrine water was employed instead of Thames water.

(Description of experiments, see pp. 465 *et seq.*; summary of conclusions, pp. 476—486.)

5. A further series of experiments made with the same sample of Loch Katrine water, only introducing a much larger number of typhoid bacilli into a given volume of water; in this series of experiments only unsterilised Loch Katrine water was employed.

(Description of experiments, see p. 486; summary of conclusions, p. 492.)

6. In order to compare the relative longevity of the typhoid bacillus in the more important types of potable water, a long series of experiments was carried out in which typhoid bacilli from one and the same cultivation, and in as far as possible equal numbers, were introduced into Thames water, Loch Katrine water, and the deep well water of the Kent Company respectively. Each of these waters was employed:—

(*a*) In its unsterile natural condition;
(*b*) Sterilised by steam;
(*c*) Sterilised by filtration through a porous cylinder of infusorial earth.

In this series of experiments the influence of rest or agitation was also studied.

(Description of experiments, see pp. 493 *et seq.*; summary of conclusions, pp. 516—518.)

7. A series of experiments made in order to ascertain whether the bactericidal properties of unsterilised surface water can be artificially induced by inoculating steam-sterilised Thames water with a few drops of unsterilised Thames water, and thus giving rise to a bacterial population in the previously sterile water.

(Description of experiments, see pp. 519 *et seq.*; summary of conclusions, pp. 528—530.)

8. Further experiments on the addition of common salt to typhoid-infected Thames water, both sterile and unsterile, with a view to confirming or contradicting the results of the experiments referred to under No. 3 above.

(Description of experiments, see pp. 530 *et seq.*; summary of conclusions, pp. 535—536.)

9. Experiments made to ascertain whether the typhoid bacillus and the *Bacillus coli communis*, multiply in potable water which is very highly charged with vegetable matter (peaty, upland, surface water).

(Description of experiments, see pp. 536 *et seq.*; summary of conclusions, p. 538.)

10. Experiments made to ascertain whether the typhoid bacillus can, by prolonged preliminary culture in more and more diluted media, be trained for aquatic life in potable water.

(Description of experiments, see pp. 539 *et seq.*; summary of conclusions, p. 541.)

For the detailed conclusions arrived at from the results of these several series of experiments, the reader is referred to the summaries which are appended to the descriptions of each series of experiments, as indicated above, whilst the general conclusions which arise out of the entire investigation may be summarised as follows :—

Summary of Conclusions.

1. Typhoid bacilli from ordinary agar-agar- and gelatine-cultures on being introduced into steam-sterilised potable water in such numbers as not to materially alter the composition of the latter undergo no multiplication. This result was uniformly obtained irrespectively of whether surface water like that of the Thames, which has received the drainage of manured land, or upland surface water like that of Loch Katrine, the organic matter in which is very similar in absolute

amount to that in Thames water, but almost exclusively derived from vegetable sources (peat); or, again, other upland surface water much more highly impregnated with peaty matter; or, lastly, deep well water containing the merest traces of organic matter, was employed.

In all cases, of course, special precautions were taken to prevent, as far as possible, the importation of culture-material along with the bacilli.

2. By first submitting the typhoid bacilli to prolonged culture in more and more aqueous media, and then introducing them into steam-sterilised Thames water, slight but distinct multiplication of the typhoid bacilli was observed, and, although, perhaps, by this method of training the typhoid bacilli had become more adapted to aquatic life, it appears probable that the multiplication observed took place at the expense of the minute quantity of culture-material necessarily introduced with them; for on transferring some of this infected water in which multiplication had taken place to a larger volume of the same steam-sterilised Thames water, no further multiplication was found to occur, showing that the organic matter belonging to the steam-sterilised Thames water itself was not capable of ministering to the growth and proliferation even of these specially educated typhoid bacilli.

3. Although no instance of multiplication of the introduced typhoid bacilli in these steam-sterilised potable waters was observed, on the other hand the bacilli were found to be possessed of very considerable longevity in them, thus:—

Description of steam-sterilised water.	Duration of life of typhoid bacillus.	
Thames water (11.5.1893) kept at 6—8° C.	Upwards of 76 days	Still just recognisable.
" " " 19° C.	" "	
Loch Katrine water (4.7.1893) kept at 6—8° C.	Upwards of 21 days	Only a small number of typhoid bacilli was introduced into these waters.
Loch Katrine water (4.7.1893) kept at 19° C.	Between 13 and 17 days	
Thames water (19.10.1893) kept at 9—12° C.	Between 32 and 39 days	Typhoid bacilli from one and the same source and in the same numbers were introduced into each of these waters.
Loch Katrine water (19.10.1893) kept at 9—12° C.	Upwards of 51 days	
Deep well water (19.10.1893) kept at 9—12° C.	Between 20 and 32 days	
Thames water (16.1.1894) kept at 9—12° C.	Upwards of 48 days (still abundantly present).	
Peaty upland surface water (17.10.1893) kept at 9—12° C.	Upwards of 24 days (still abundantly present).	

In no case was the duration of vitality a very limited one; its exact length in any particular water is doubtless dependent on the initial vitality of the bacilli and the numbers in which they are introduced. In the strictly comparative experiment on steam-sterilised Thames, Loch Katrine, and deep well water, it is seen that the longevity of the typhoid bacillus is distinctly greatest in the Loch

Katrine, and least in the deep well water, and intermediate between the two in Thames water. Of these three waters, also, the Loch Katrine contains the most, the deep well the least, and the Thames an intermediate amount of organic matter. Not improbably these circumstances are connected together.

4. The experiments distinctly show that in these steam-sterilised potable waters a summer temperature of 19° C. is more prejudicial than a winter temperature of 6—8° C. to the duration of life of the typhoid bacillus.

5. Inasmuch as the numerical estimation of typhoid bacilli in unsterile potable waters is practically impossible, the duration of life of the typhoid bacilli in such waters has alone been made the subject of study. This enquiry has involved an enormous amount of labour, as the certain detection, by means of the special methods employed, of the typhoid bacillus, even in a single specimen of water, may entail work extending over several weeks. The duration of life of the typhoid bacilli introduced into the various unsterile waters in the several series of experiments was as follows :—

Description of unsterile water.	Duration of life of typhoid bacillus.	
Thames water (11.5.1893) kept at 6—8° C.	Between 25 and 34 days.	
" " " 19° C.		
Loch Katrine water (4.7.1893) kept at 6—8° C.	Upwards of 17 days	Only a small number of typhoid bacilli was introduced into these waters.
Loch Katrine water (4.7.1893) kept at 19° C.	Between 4 and 11 days	
Loch Katrine water (7.7.1893) kept at 6—8° C.	Upwards of 14 days, after which no further examinations were made. (A much larger number of typhoid bacilli was introduced in this than in the above experiments.)	
Loch Katrine water (7.7.1893) kept at 19° C.		
Thames water (19.10.1893) kept at 9—12° C.	Between 9 and 13 days	Typhoid bacilli from one and the same source and in the same numbers were introduced into each of these waters.
Loch Katrine water (19.10.1893) kept at 9—12° C.	Between 19 and 33 days	
Deep well water (19.10.1893) kept at 9—12° C.	Between 33 and 39 days	
Thames water (16.1.1894) kept at 9—12° C.	Between 20 and 27 days.	

On comparing this table with that given under No. 3 above, it will be seen that in all cases, excepting one, the duration of life of the typhoid bacillus was greater, and often much greater, in the steam-sterilised than in the corresponding waters unsterilised. The single exception to this general rule was in the case of the deep well water in which the typhoid bacilli lived about the same length of time, irrespectively of whether the water was sterile or not.

The table also shows that, as in the case of the steam-sterilised waters, the exact length of time that the typhoid bacilli endured residence in one and the same type of unsterilised water was subject

to great variations in the different experiments, doubtless largely in consequence of the different initial vitality of the typhoid bacilli employed, and also, doubtless, in consequence of the different numbers in which they were introduced in the several series of experiments.

Of principal interest is the comparative experiment made with Thames, Loch Katrine, and deep well water, in which typhoid bacilli from one and the same source, and in the same numbers, were introduced into these three types of water, and in which the duration of life was found to be shortest in the Thames water (9—13 days), longest in the deep well water (33—39 days), and intermediate in the Loch Katrine water (19—33 days). This result is of very great practical importance as indicating the greater danger of typhoid bacilli gaining access to deep well than to surface water. This danger is, in actual practice, further enhanced by the fact that well water is almost invariably consumed without storage, whilst surface-waters are often stored for days or weeks, and in the case of upland surface water the storage frequently extends over many months.

The effect of temperature on the duration of life of the typhoid bacillus was well illustrated in the series of experiments with Loch Katrine water (4.7.1893), in which it was found that at 19° C. the typhoid bacilli had already disappeared in 4 to 11 days, whilst at 6—8° C. they were alive for upwards of 17 days in the same water.

The effect of agitation or rest on the typhoid bacilli in these waters was not very pronounced, but the evidence on the whole, both in the case of the sterilised and unsterilised waters, goes to show that the agitation or aëration of the water is unfavourable to the typhoid bacilli, and that in the unsterilised waters it occasions a more rapid multiplication of the water bacteria.

6. The greater bactericidal power of unsterilised than steam-sterilised surface waters is not apparently due to the multiplication of the water bacteria in the unsterile waters, bringing about a competition or "struggle for existence" between these aquatic forms and the typhoid bacilli, but rather to the elaboration of products by these aquatic bacteria (and very possibly also by other vegetable life present in surface waters) which are inimical and prejudicial to the welfare of the typhoid bacilli.

Thus, in the typhoid-infected unsterilised deep well water an enormously greater multiplication of the common water bacteria took place than in the unsterile Thames and Loch Katrine waters; yet, notwithstanding the typhoid bacilli not only lived much longer in this unsterile deep well water than in the unsterile Thames and Loch Katrine waters, but there was practically no difference between the duration of life of the typhoid bacilli in the sterile and unsterile deep well water respectively.

Of course it may be urged that the unsterile deep well water possibly does not contain those water bacteria which are particularly fitted for entering into successful competition with the typhoid bacilli, and that perhaps such water bacteria are only to be found in the unsterile surface waters.

7. The series of experiments summarised in the following table show that unsterile surface water, like that of the Thames, possesses bactericidal powers irrespectively of any further multiplication of its contained water bacteria, thus:—

Uninfected unsterilised Thames water, kept at 9—12° C., exhibited but little change in the number of its contained bacteria over the period of five weeks from 16.1.1894 to 24.2.1894. (The numbers only varied from 5500—2825 per 1 c.c.)

The same unsterilised Thames water, infected with about 170,000 typhoid bacilli per 1 c.c., exhibited a continuous decline in the total number of bacteria present in it over the same period.

The same Thames water, after sterilisation by steam, was infected with upwards of 100,000 typhoid bacilli per 1 c.c., and at the end of the same period (16.1.1894—24.2.1894) there were still upwards of 5000 typhoid bacilli per 1 c.c. present.

The same typhoid-infected steam-sterilised water was inoculated with a few drops of unsterile Thames water to communicate to it the Thames-water bacteria, and the latter underwent very extensive multiplication in this water. Notwithstanding, the typhoid bacilli lived between thirty-four and forty-one days in this water, whilst in the unsterile Thames water, in which no multiplication of the water bacteria took place, they only lived between twenty and twenty-seven days.

This shorter duration of life of the typhoid bacilli in naturally unsterile Thames water than in that rendered unsterile by inoculation I attribute to the circumstance that in the naturally unsterile Thames water countless generations of water bacteria must have flourished before the water is made the subject of experiment at all, and it must, therefore, be more or less saturated with those bacterial products which are prejudicial to the vitality of the typhoid bacillus, and which, in fact, frequently hamper or even inhibit the further multiplication of the water bacteria themselves.

Thus it is obvious that the unsterile water in question was already, at the outset of the experiment, in such a condition as to prevent any multiplication of its own water bacteria, whilst, after it had been steam sterilised, the same bacteria multiplied abundantly in it. But again, at the outset of the experiment the unsterile water was in such a condition as to cause a comparatively rapid disappearance of the introduced typhoid bacilli, whilst after steam sterilisation it only became again endowed with this power of destroying the typhoid

bacilli when the introduced water bacteria had undergone extensive multiplication.

8. The addition of common salt to unsterile Thames water, in the proportion of 0·1, 1, and 3 per cent., causes the enormous multiplication of the water bacteria present, the most striking result in this respect being obtained with the largest addition of salt.

9. The addition of common salt to typhoid-infected unsterile Thames water diminishes the duration of life of the typhoid bacilli in this water, thus:—

	Duration of life of typhoid bacillus.
Unsterile Thames water kept at 6—8° C.	Between 25 and 34 days.
" " " 19° C.	(Between 25 and 34 days.)
Ditto with 0·1 per cent. salt, kept at 6—8° C.	" 25 " 33 "
" " " 19° C.	Less than 18 days.
Ditto with 1 per cent. salt, kept at 6—8° C.	" "
" " " 19° C.	" "
Ditto with 3 per cent. salt, kept at 6—8° C.	" "
" " " 18° C.	" "

10. This more rapid disappearance of the typhoid bacilli in the unsterile Thames water to which salt was added cannot be wholly, but only in part, attributed to the resulting multiplication of the water bacteria, as the addition of salt in similar proportion to typhoid-infected steam-sterilised Thames water also caused an exceedingly rapid disappearance of the typhoid bacilli, although the disappearance was not so rapid as in the same water to which a few drops of unsterile Thames water had been added, and in which, therefore, a great multiplication of the water bacteria took place, thus:—

	Duration of life of typhoid bacilli.
Steam-sterilised Thames water with 1 per cent. salt, kept at 9—12° C.	Between 12 and 19 days.
Ditto, to which also a few drops of unsterile Thames water were added, and in which extensive multiplication of the so-introduced water bacteria took place	Less than 5 days.

11. The *Bacillus coli communis*, taken from ordinary agar-agar cultures and introduced into steam-sterilised Thames water, undergoes considerable multiplication, when under precisely similar conditions the typhoid bacillus does not multiply.

The behaviour of the *B. coli communis* in the steam-sterilised waters may be summarised as follows:—

Description of steam-sterilised water.	Duration of life of *B. coli communis.*
Thames water (11.5.1893) kept at 6—8° C. Thames water (11.5.1893) kept at 19° C.	Still abundantly present, after considerable multiplication, on the 75th day.
Loch Katrine water (4.7.1893) kept at 6—8° C. Loch Katrine water (4.7.1893) kept at 19° C.	Between 14 and 17 days. The coli bacilli were introduced in this case in much smaller numbers than in that of the Thames water above. No multiplication was observed.
Very peaty water (30.10.1893) kept at 9—12° C.	Upwards of 24 days. Still present in undiminished numbers after very slight multiplication.

12. The *Bacillus coli communis* introduced into unsterile water persists in the living state for a much longer period than the typhoid bacillus. Thus:—

	Duration of life of the *B. coli communis.*
Unsterile Thames water (11.5.1893) kept at 6—8° C. Unsterile Thames water (11.5.1893) kept at 19° C.	Upwards of 40 days, and doubtless much longer, but no further examinations made.
Unsterile Loch Katrine water (4.7.1893) kept at 6—8° C. Unsterile Loch Katrine water (4.7.1893) kept at 19° C.	Upwards of 17 days. No further examinations made.

13. In the numerous experiments made on the behaviour of the typhoid bacillus and of the *B. coli communis* in water (Thames, Loch Katrine, deep well, and peaty water) which had been sterilised by filtration through cylinders of porous earthenware and baked infusorial earth, a most remarkably rapid disappearance of both bacilli was observed in all cases, excepting that of the Loch Katrine water. Further experiments will have to be made before any definite conclusions can be drawn from these unexpected results.

Appendix.

The Behaviour in Potable Water of Anthrax Bacilli taken directly from the Animal Body.

By Percy Frankland, Ph.D., B.Sc., F.R.S., and Charles Templeman, M.D., B.Sc.

In the 2nd Report to the Royal Society Water Research Committee, "On the Vitality and Virulence of the *Bacillus anthracis* and its Spores in Potable Water," the enquiry was mainly confined to the deportment either of anthrax spores alone, or of such mixtures of bacilli and spores which are found in the usual cultivations of anthrax

on artificial media. It is, however, obviously a matter of great importance to ascertain how anthrax bacilli, entirely free from spores, as they are found in the tissues of animals which have succumbed to this disease, behave when they are introduced into potable water. There is the more urgency for this investigation, as it was pointed out in the introduction to the Report that the experiments previously undertaken by others in this direction have led to highly discordant results.

The experiments which we have made on this subject were incidental to another investigation, on which we propose reporting later on, but as these experiments should have been made in connection with the 2nd Report, had time and opportunity permitted, we are bringing them forward now to fill up without further delay the hiatus in that Report.

First Series of Experiments.

The spleen of a white mouse, dead of anthrax, was excised under the usual aseptic precautions, and transferred to a small sterile bottle containing 20 c.c. of sterilised tap water, in which it was completely broken up by bruising with a sterile glass rod. More sterile water about 50 c.c. in all, was added to the bottle, and the whole violently shaken for fifteen minutes, so as to ensure even distribution of the bacilli throughout the water. 10 c.c. of this water attenuation were then added to about 400 c.c. of steam-sterilised Dundee water and thoroughly mixed, after which this infected water was distributed amongst a number of sterile tubes plugged with cotton wool. This infected water was submitted to plate cultivation on the same day, with the following results:—

Tube 16	$\frac{5}{9}$ c.c.	water	yielded	8,190*	anthrax colonies	per 1 c.c.
	$\frac{2}{9}$	,,	,,	7,871	,,	,,
Tube 17	$\frac{10}{25}$	,,	,,	8,235	,,	,,
	$\frac{4}{25}$	,,	,,	8,325	,,	,,
Tube 18	$\frac{5}{10}$	,,	,,	11,712	,,	,,
	$\frac{2}{10}$	,,	,,	8,100	,,	,,
Tube 19	$\frac{5}{12}$	,,	,,	10,248	,,	,,
	$\frac{2}{12}$	,,	,,	12,810	,,	,,
Tube 20	$\frac{5}{10}$	,,	,,	9,408	,,	,,
	$\frac{2}{10}$	,,	,,	6,050	,,	,,
				90,949		
			Average......	9,095		

Thus, on the day of infection the water contained about 9000 anthrax bacilli per 1 c.c.

* All these plates were incubated for 5 days at 18—20° C.

The tubes containing this infected water were kept in a dark cupboard at 12° C., and submitted to plate cultivation at intervals, thus:—

Plate cultures prepared.	Number of tube.	Number of days plates were incubated at 18—20° C.	Volume of water used for plate cultivation.	Number of anthrax colonies per 1 c.c. of water.
			c.c.	
2 days after infection	19	9	$\frac{5}{10}$	6
			$\frac{2}{10}$	10
,,	20	,,	$\frac{5}{10}$	236
			$\frac{2}{10}$	135
5 days after infection	19	9	$\frac{5}{10}$	All the plates were free from anthrax.
			$\frac{2}{10}$	
,,	20	,,	$\frac{5}{10}$	
			$\frac{2}{10}$	

These tubes were again examined on two subsequent occasions and again yielded sterile plates.

Thus these anthrax bacilli, taken directly from the dead mouse and introduced into sterile Dundee water in such large numbers as 9000 *per* 1 *c.c. of water, all died within five days, the water being kept at* 12° C.

Second Series of Experiments.

In this series of experiments the anthrax bacilli from the dead animal were introduced both into steam-sterilised Thames and steam-sterilised Dundee water respectively, and these waters were preserved at different temperatures, in order to ascertain the influence of this factor on the result.

The spleen of a white mouse, which had died of anthrax twenty-eight hours after inoculation, was broken up with sterilised tap water in the same way as already described above, and with this water attenuation larger volumes of sterile Thames and Dundee waters respectively were infected, and these infected waters were then distributed amongst a number of sterile tubes plugged with cotton wool. On the day of infection some of these tubes were submitted to plate cultivation, in order to ascertain the number of anthrax bacilli introduced, thus:—

Dundee water	Tube No. 1	$\frac{5}{10}$ c.c. water	yielded	4840	anthrax colonies	per 1 c.c.
		$\frac{2}{10}$,,	,,	4860	,,	,,
	,, 2	$\frac{5}{11}$,,	,,	6897	,,	,,
		$\frac{2}{11}$,,	,,	7722	,,	,,
Thames water	,, 7	$\frac{5}{10}$,,	,,	6136	,,	,,
		$\frac{2}{10}$,,	,,	5875	,,	,,
	,, 8	$\frac{5}{12}$,,	,,	6480	,,	,,
		$\frac{2}{12}$,,	,,	6552	,,	,,

The average number of anthrax bacilli in these infected Thames and Dundee waters at the outset was, therefore, about 6000 per 1 c.c. Some of these tubes were then placed in a refrigerator at 5° C., others were kept in a cupboard at 13° C., whilst others were put in an incubator at 19° C. The water in these tubes, kept at the different temperatures specified, was submitted to plate cultivation at intervals, with the following results:—

Water kept at 5° C.

Plate cultures prepared.	Description of water.	Number of tube.	Number of days plates were incubated at 18—20° C.	Volume of water used for plate cultivation.	Number of anthrax colonies per 1 c.c. of water.
				c.c.	
1st day after infection	Thames	9	6	$\frac{5}{10}$	1028
				$\frac{2}{10}$	2470
		10	,,	$\frac{10}{25}$	2550
				$\frac{4}{25}$	2031
,,	Dundee	3	,,	$\frac{10}{10}$	3386
				$\frac{4}{10}$	3733
		4	,,	$\frac{5}{10}$	3456
				$\frac{2}{10}$	3335
2 days after infection	Thames	9	6	$\frac{5}{11}$	834
				$\frac{2}{11}$	660
		10	,,	$\frac{5}{10}$	860
				$\frac{2}{10}$	485
,,	Dundee	3	,,	$\frac{5}{10}$	1624
				$\frac{2}{10}$	1995
		4	,,	$\frac{10}{10}$	1864
				$\frac{4}{10}$	1497
5 days after infection	Thames	9	6	$\frac{5}{13}$	None
				$\frac{2}{13}$	,,
		10	,,	$\frac{5}{10}$	,,
				$\frac{2}{10}$	,,
,,	Dundee	3	,,	$\frac{10}{20}$	134
				$\frac{4}{20}$	235
		4	,,	$\frac{10}{31}$	None
				$\frac{4}{31}$	,,
14 days after infection	Thames	9	6	1·0	None
			,,	0·5	,,
		10	,,	1·0	,,
			,,	0·5	,,
,,	Dundee	3	,,	1·0	,,
			,,	0·5	,,
		4	,,	1·0	,,
			,,	0·5	,,

Thus, in the Thames water maintained at 5° C. *the anthrax bacilli (introduced to the number of* 6000 *per* 1 *c.c.) had already undergone very*

considerable diminution in numbers on the day following their introduction; after two days the numbers had still further diminished, whilst five days after introduction they were no longer discoverable at all. The fate of the anthrax bacilli in the Dundee water was quite similar, their disappearance being, however, a little less rapid; thus a few bacilli were still present in one of the two tubes on the fifth day after infection.

The following table exhibits the results obtained with the same waters kept at 13° C.:—

Water kept at 13° C.

Plate cultures prepared.	Description of water.	Number of tube.	Number of days plates were incubated at 18—20° C.	Volume of water used for plate cultivation.	Number of anthrax colonies per 1 c.c. of water.
				c.c.	
1st day after infection	Thames	7	6	$\frac{5}{10}$	2640
				$\frac{2}{10}$	2870
		8	”	$\frac{5}{10}$	3564
				$\frac{2}{10}$	3505
”	Dundee	1	”	$\frac{5}{11}$	4422
				$\frac{2}{11}$	4048
		2	”	$\frac{5}{6}$	3331
				$\frac{2}{6}$	3153
2 days after infection	Thames	7	6	$\frac{5}{10}$	1690
				$\frac{2}{10}$	—
		8	”	$\frac{5}{10}$	2196
				$\frac{2}{10}$	2035
”	Dundee	1	”	$\frac{5}{6}$	2442
				$\frac{2}{6}$	2848
		2	”	$\frac{5}{10}$	2806
				$\frac{2}{10}$	2785
5 days after infection	Thames	7	6	$\frac{5}{15}$	910
				$\frac{2}{15}$	—
		8	”	$\frac{5}{10}$	1096
				$\frac{2}{10}$	1240
”	Dundee	1	”	$\frac{5}{11}$	—
				$\frac{2}{11}$	1427
		2	”	$\frac{5}{10}$	2283
				$\frac{2}{10}$	1965
14 days after infection	Thames	7	6	1·0	None
			”	0·5	”
		8	”	1·0	”
			”	0·5	”
”	Dundee	1	”	1·0	”
			”	0·5	”
		2	”	1·0	”
			”	0·5	”

Thus at the higher temperature of 13° C., the anthrax bacilli were markedly more persistent than at 5° C., although they had in all cases

become largely reduced in numbers by the fifth day after their introduction into the water, and by the fourteenth day they had all disappeared.

In the following table are recorded the results which were obtained with the same waters maintained throughout at a temperature of 19° C. :—

Water kept at 19° C.

Plate cultures prepared.	Description of water.	Number of tube.	Number of days plates were incubated at 18—20° C.	Volume of water used for plate cultivation.	Number of anthrax colonies per 1 c.c. of water.
				c.c.	
1st day after infection	Thames	11	6	$\frac{5}{10}$	3,400
				$\frac{2}{10}$	3,200
		12	”	$\frac{5}{5}$	2,786
				$\frac{2}{5}$	2,830
”	Dundee	5	”	$\frac{5}{12}$	6,110
				$\frac{2}{12}$	4,908
		6	”	$\frac{5}{12}$	5,327
				$\frac{2}{12}$	5,718
2 days after infection	Thames	11	6	$\frac{5}{10}$	3,530
				$\frac{2}{10}$	4,420
		12	”	$\frac{5}{12}$	4,066
				$\frac{2}{12}$	4,253
”	Dundee	5	”	$\frac{10}{25}$	5,125
				$\frac{4}{25}$	10,000
		6	”	$\frac{5}{10}$	5,200
				$\frac{2}{10}$	5,745
5 days after infection	Thames	11	6	$\frac{10}{15}$	30,428
				$\frac{4}{15}$	36,020
		12	”	$\frac{5}{10}$	32,448
				$\frac{2}{10}$	28,194
”	Dundee	5	”	$\frac{5}{10}$	46,200
				$\frac{2}{10}$	45,825
		6	”	$\frac{5}{12}$	32,370
				$\frac{2}{12}$	25,140
14 days after infection	Thames	11	6	$\frac{10}{27}$	—
				$\frac{4}{27}$	207,238
		12	”	$\frac{10}{25}$	—
				$\frac{4}{25}$	163,750
”	Dundee	5	”	$\frac{10}{25}$	—
				$\frac{4}{25}$	66,581
		6	”	$\frac{5}{12}$	—
				$\frac{2}{12}$	54,300
42 days after infection	Thames	11	3	$\frac{10}{25}$	—
				$\frac{4}{25}$	99,937
		12	”	$\frac{5}{15}$	109,590
				$\frac{2}{15}$	102,180
”	Dundee	5	”	$\frac{5}{10}$	—
				$\frac{2}{10}$	66,240
		6	”	$\frac{5}{12}$	51,227
				$\frac{2}{12}$	49,610

Thus at the temperature of 19° C., the behaviour of the anthrax bacilli in these same waters was entirely different; far from their undergoing rapid diminution in numbers followed by early disappearance, they only exhibited slight diminution during the first few days after their introduction, upon which there followed an enormous multiplication. This multiplication was already observable, in the case of one tube, on the second day after infection, whilst on the fifth day it was very pronounced in all the tubes, and on the fourteenth day the numbers reached were very large, remaining practically unaltered even on the forty-second day.

The explanation which naturally suggests itself for this entirely different behaviour of the anthrax bacilli at the higher and the lower temperature respectively, is that at the higher temperature of 19° C. the anthrax bacilli can form spores, whilst at the lower temperatures this sporulation cannot take place. With the appearance of the spores, however, the longevity of anthrax in sterile potable water becomes, as was shown in the Second Report, practically indefinite.

In order to test the validity of this hypothesis that sporulation had taken place in the waters kept at 19° C., the following experiments were made:—

(1.) 1 c.c. of the contents of Tube 11 (Thames water, see table above) was kept at 70° C. for ten minutes, in order to destroy anthrax bacilli; on subsequent plate cultivation, innumerable anthrax colonies were obtained.

(2.) A similar experiment made with 1 c.c. of the contents of Tube 12 (Thames water, see table above) gave exactly the same result.

(3.) A similar experiment made with 1 c.c. of the contents of Tube 5 (Dundee water, see table above) gave the same result, innumerable anthrax colonies being obtained on the plate.

(4.) A similar experiment made with 1 c.c. of the contents of Tube 6 (Dundee water, see table above) gave 23,352 anthrax colonies.

Thus in the case of all these waters kept at 19° C., it is evident that practically the whole of the anthrax microbes present at the end of forty-two days were there in the condition of spores, showing as they did no appreciable diminution in numbers by the process of heating to 70° C. for ten minutes.

These experiments show, then, very clearly that the fate of virulent anthrax bacilli passing from an anthrax victim into potable water will be dependent on the temperature of the latter; if the temperature of the water is below that at which sporulation of anthrax can take place, then the bacilli will perish in the course of a few days; whilst if the temperature is high enough to admit of sporulation,

then anthrax spores will be formed, and these, as is now well known, may persist in a living and virulent condition for an almost indefinite period of time—for months and probably even for years.

From these experiments it further appears that the lowest temperature at which spore formation of anthrax in water will take place lies somewhere between 15° and 19° C. According to Koch, the lowest temperature at which anthrax spores are obtained is 16° C., and the most advantageous temperature for the production of the hardiest spores is 20—25° C.

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PROCEEDINGS OF

THE ROYAL SOCIETY.

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CONTENTS.

Price One Shilling.

November 30, 1894.

Report of Magnetical Observations at Falmouth Observatory for the Year 1893. Latitude 50° 9′ 0″ N. and Longitude 5° 4′ 35″ W.; height, 167 feet above mean sea-level.[*]

These observations have been made by instruments purchased from the Government Grant Fund administered by the Royal Society.

The Observatory having been comparatively recently established, the Vertical Force self-recording instrument is not yet in thorough working order. It is hoped in future to publish complete records of all three elements.

Photographic curves of Magnetic Declination and of Horizontal Force variations have been taken regularly throughout the past year, and the magnets have worked satisfactorily.

The scale values of the instruments were determined in April last. The following values of the ordinates of the photographic curves were then found:—

Declination, 1 cm. = 0° 11′·7.

Bifilar, April 7th, for 1 cm. δ H., = 0·00055 C.G.S. unit.

This latter value not being in accordance with the prescribed standard scale, the sensibility of the magnet was increased, and a second series of deflections made on April 10th, when the value was determined for 1 cm. δ H. = 0·00050 C.G.S. unit.

No violent magnetic disturbances have been recorded during the year; the principal movements occurred on the following dates:— February 5, March 26, July 16, August 6, 7, 18, and November 1, 2.

Observations with the Absolute Instruments have been made monthly, of which the following is a summary:—

Determinations of	Horizontal Intensity,	34.
„	Inclination,	35 sets of four.
„	absolute Declination,	33.

The results in the following tables, Nos. 1, 2, 3, 4, are deduced from the magnetograph curves which have been standardised by observations of deflection and vibration. These were made with the Collimator Magnet marked 66A, and the Declinometer Magnet marked 66C in the Unifilar Magnetometer by Elliott Brothers, of London. Table No. 5 is deduced from these observations.

[*] The records of the Falmouth Magnetic Observatory have hitherto been published in the 'Journal of the Royal Cornwall Polytechnic Society.' The committee of management having obtained leave to communicate their annual magnetic report to the Royal Society, it will henceforward be printed in the 'Proceedings.' The results are worked up in the same way as those obtained at Kew, and the reports of the two observatories will in future appear simultaneously.—R.

Table I.—Hourly Means of Declination, at the Falmouth
on five selected quiet Days in

(19° + West.)

Hours	Mid.	1	2	3	4	5	6	7	8	9	10	11
Winter.												
1893. Months.	′	′	′	′	′	′	′	′	′	′	′	′
Jan. ..	7·2	7·7	8·4	9·1	9·0	9·0	10·2	9·1	8·9	8·7	9·2	11·0
Feb. ..	7·3	7·3	7·5	7·4	7·2	6·9	6·5	6·0	5·6	5·8	7·0	9·4
March .	7·3	7·2	6·8	6·7	6·2	5·7	5·5	5·1	3·6	3·1	4·5	8·0
Oct. ..	3·6	3·7	3·5	3·6	3·9	3·8	3·4	2·3	0·6	0·2	1·7	4·9
*Nov. ..	3·9	4·7	4·4	4·4	4·4	4·4	4·4	3·5	2·9	2·2	3·6	6·7
Dec. ..	4·7	5·5	5·8	6·0	5·7	5·7	5·5	5·5	5 2	4·6	4·4	6·4
Means	5·7	6·0	6·1	6·2	6·1	5·9	5·9	5·2	4·5	4·1	5·1	7·7
Summer.												
	′	′	′	′	′	′	′	′	′	′	′	′
April ..	6·8	6·8	6·8	6·6	6·4	6·1	5·2	3·7	2·1	1·8	2·7	6·5
†May ..	5·9	6·5	6·6	6·0	5·5	3·9	2·6	0·8	0·3	0·6	2·8	6·4
June ..	6·1	5·6	5·5	5·3	5·0	3·3	1·1	0·7	0·3	0·7	3·0	5·9
July ..	4·5	4·6	4·2	4·1	3·7	2·4	0·8	−0·6	−0·9	−0·2	2·4	5·9
Aug. ..	4·5	4·6	4·3	4·1	3·4	2·6	1·0	−0·5	−0·5	0·3	2·8	6·4
Sept. ..	3·2	3·2	3·5	2·6	2·2	2·2	1·2	0·3	−0·6	0·3	2·7	5·9
Means	5·2	5·2	5·1	4·8	4·4	3·4	2·0	0·7	0·1	0·6	2·7	6·2

* Mean derived from 7th, 11th, and 21st. † Mean of four days, 2nd, 14th, 21st, and 28th.

Table II.—Solar Diurnal Range of the Falmouth

Hours	Mid.	1	2	3	4	5	6	7	8	9	10	11
Summer mean.												
	′ −0·6	′ −0·6	′ −0·7	′ −1·1	′ −1·5	′ −2·4	′ −3·9	′ −5·2	′ −5·8	′ −5·3	′ −3·1	′ +0·4
Winter mean.												
	′ −1·5	′ −1·2	′ −1·1	′ −1·0	′ −1·1	′ −1·3	′ −1·3	′ −2·0	′ −2·7	′ −3·1	′ −2·1	′ +0·5
Annual mean.												
	′ −1·0	′ −0·9	′ −0·9	′ −1·0	′ −1·3	′ −1·8	′ −2·6	′ −3·6	′ −4·2	′ −4·2	′ −2·6	′ +0·4

NOTE.—When the sign is + the magnet

Observatory determined from the Magnetograph Curves each Month during the Year 1893.

Noon	1	2	3	4	5	6	7	8	9	10	11	Mid.
Winter.												
′	′	′	′	′	′	′	′	′	′	′	′	′
12·6	14·0	13·6	12·7	12·5	11·9	11·3	10·7	10·0	9·5	9·2	8·7	8·8
11·4	12·5	12·6	11·5	10·2	9·2	8·7	8·6	8·2	8·0	7·6	7·3	7·4
12·0	14·9	15·5	13·9	11·8	9·2	8·7	8·1	7·6	7·4	7·5	7·5	7·4
8·5	10·2	10·2	11·0	7·4	6·2	5·3	4·8	4·5	3·7	3·4	3·6	3·2
9·3	10·6	9·9	8·5	7·7	6·7	6·7	6·6	5·2	4·7	4·3	4·5	5·1
8·6	8·7	9·8	9·5	8·8	7·7	7·3	6·9	5·6	5·2	5·0	5·1	4·2
10·4	11·8	11·9	11·2	9·7	8·5	8·0	7·6	6·8	6·4	6·2	6·2	6·0
Summer.												
′	′	′	′	′	′	′	′	′	′	′	′	′
10·8	14·0	15·1	13·7	11·5	9·7	7·9	7·5	7·4	7·3	7·4	7·0	6·9
11·1	13·1	13·7	12·2	9·9	8·3	7·1	6·6	6·6	6·6	6·6	6·6	6·8
9·8	12·2	12·4	11·2	9·8	8·2	7·2	6·1	5·8	5·5	5·9	5·7	5·9
9·3	10·9	11·9	11·6	9·7	7·4	5·9	4·7	4·6	4·6	5·3	5·1	4·8
10·1	12·7	12·7	11·1	8·9	6·6	4·9	4·7	4·5	4·3	4·2	4·3	4·5
9·8	11·8	11·8	10·2	7·7	6·0	5·0	4·6	4·2	4·2	3·9	3·4	3·2
10·2	12·5	12·9	11·7	9·6	7·7	6·3	5·7	5·5	5·4	5·5	5·3	5·3

Declination as derived from Table I.

Noon	1	2	3	4	5	6	7	8	9	10	11	Mid.
Summer mean.												
′	′	′	′	′	′	′	′	′	′	′	′	′
+4·4	+6·7	+7·2	+5·9	+3·8	+1·9	+0·5	−0·1	−0·3	−0·4	−0·3	−0·5	−0·5
Winter mean.												
′	′	′	′	′	′	′	′	′	′	′	′	′
+3·2	+4·6	+4·7	+4·0	+2·5	+1·3	+0·8	+0·4	−0·4	−0·8	−1·0	−1·0	−1·2
Annual mean.												
′	′	′	′	′	′	′	′	′	′	′	′	′
+3·8	+5·6	+5·9	+4·9	+3·1	+1·6	+0·6	+0·2	−0·3	−0·6	−0·6	−0·7	−0·8

points to the west of its mean position.

Table III.—Hourly Means of the Horizontal Force at Falmouth (corrected for Temperature), on five

0·18000 + (C.G.S. units.)

Hours	Mid.	1	2	3	4	5	6	7	8	9	10	11
						Winter.						
1893. Months.												
Jan. ..	430	430	433	435	437	439	440	440	439	435	429	423
Feb. ..	469	469	469	469	470	470	470	470	467	456	446	444
March .	447	446	446	444	444	444	445	441	433	421	412	411
Oct. ..	472	469	469	471	471	470	468	467	458	449	440	435
Nov. ..	460	460	461	459	455	462	463	463	459	446	434	430
Dec. ..	455	454	454	457	458	460	464	464	461	456	449	443
Means	456	455	455	456	456	457	458	457	453	444	435	431
						Summer.						
April ..	473	471	471	472	471	472	475	475	470	459	440	429
May ..	471	471	472	470	470	468	464	457	444	434	429	429
June ..	478	475	474	474	473	473	468	459	452	446	442	441
July ..	464	462	460	459	460	460	456	448	439	429	422	422
Aug. ..	464	464	464	464	465	463	459	449	439	430	422	422
Sept. ..	465	463	464	461	463	459	456	451	442	429	424	427
Means	469	468	467	467	467	466	463	456	444	438	430	428

Table IV.—Diurnal Range of the Falmouth

(C.G.S. units.)

Hours	Mid.	1	2	3	4	5	6	7	8	9	10	11
						Summer mean.						
	+·00008	+·00007	+·00006	+·00006	+·00006	+·00003	+·00002	−·00005	−·00017	−·00023	−·00031	−·0002[illegible]
						Winter mean.						
	+·00004	+·00003	+·00003	+·00004	+·00004	+·00005	+·00006	+·00005	+·00001	−·00008	−·00017	−·0002[illegible]
						Annual mean.						
	+·00006	+·00005	+·00003	+·00005	+·00005	+·00005	+·00004	·00000	−·00008	−·00016	−·00024	−·0002[illegible]

NOTE.—When the sign is + the

Observatory as determined from the Magnetograph Curves
selected quiet Days in each Month during the Year 1893.

Noon	1	2	3	4	5	6	7	8	9	10	11	Mid.
						Winter.						
424	431	435	435	436	436	439	442	442	443	443	443	442
447	452	457	460	462	463	468	471	472	472	470	470	470
411	420	431	439	445	442	445	448	450	450	450	449	448
442	446	451	459	462	464	471	472	474	476	476	475	475
433	439	447	454	458	464	466	467	471	472	471	468	468
442	445	448	453	455	456	460	464	464	465	463	460	457
433	439	445	450	453	454	458	461	462	463	462	461	460
						Summer.						
427	434	446	461	469	474	474	476	479	476	477	476	475
431	448	460	471	476	480	482	482	483	483	479	479	481
446	454	463	470	478	484	489	493	490	488	488	484	483
431	436	443	455	462	469	475	476	471	472	469	466	465
432	443	454	459	466	466	468	473	473	474	472	469	468
434	441	451	455	455	458	464	466	470	471	471	471	467
433	443	453	462	468	472	475	478	478	477	476	474	473

Horizontal Force as deduced from Table III.

Noon	1	2	3	4	5	6	7	8	9	10	11	Mid.
						Summer mean.						
− ·00028	− ·00018	− ·00008	+ ·00001	+ ·00007	+ ·00011	+ ·00014	+ ·00017	+ ·00017	+ ·00016	+ ·00015	+ ·00013	+ ·00012
						Winter mean.						
− ·00019	− ·00013	− ·00007	− ·00002	+ ·00001	+ ·00002	+ ·00006	+ ·00009	+ ·00010	+ ·00011	+ ·00010	+ ·00009	+ ·00008
						Annual mean.						
− ·00024	− ·00016	− ·00008	− ·00001	+ ·00004	+ ·00007	+ ·00010	+ ·00013	+ ·00014	+ ·00014	+ ·00013	+ ·00011	+ ·00010

reading is above the mean.

The Inclination was observed with the Inclinometer No. 86 by Dover, of Charlton, Kent, and needles 1 and 2, which are 3½ inches in length, the results of which appear in Table VI.

The Declination and Horizontal Force values given in Tables I to IV are prepared in accordance with the suggestions made in the fifth report of the Committee of the British Association on comparing and reducing magnetic observations, and the time given is Greenwich mean time, which is 20 min. 18 sec. earlier than local time.

The following is a list of the days during the year 1893 which were selected by the Astronomer Royal, as suitable for the determination of the magnetic diurnal variations, and which have been employed in the preparation of the magnetic tables:—

January	7, 8, 15, 25, 26.
February	1, 11, 13, 26, 27.
March	10, 13, 18, 19, 20.
April..............	4, 9, 21, 22, 23.
May	2, 14, 17, 21, 28.
June	8, 13, 17, 22, 24.
July	5, 6, 10, 30, 31.
August............	1, 9, 16, 17, 27.
September	4, 7, 13, 23, 24.
October............	9, 11, 16, 21, 22.
November..........	7, 11, 15, 20, 21.
December..........	7, 13, 18, 21, 22.

The following are the principal results of the magnetic elements for the year 1893:—

Mean Westerly Declination,	19° 6′·4.
Mean Inclination,	67° 5′·3.
Mean Horizontal Force,	0·18455 C.G.S. unit.

The Declination and Horizontal Force are deduced from hourly readings of the photographic curves, and so are corrected for the diurnal variation.

The Inclination is the mean of the absolute observations, the mean time of which is noon.

In Table V, X is the mean of the absolute values observed during the month (generally three in number), uncorrected for diurnal variations and for any disturbance. Y is the mean of the products of the tangents of the Dips and the corresponding values of X.

The whole of the instruments have been maintained in good order. The Magnetic Chamber has been kept in a satisfactory state of dryness, and the Magnetic Hut in the garden has been newly roofed during the year.

EDWARD KITTO,
Magnetic Observer.

Table V.—Magnetic Intensity. Falmouth Observatory, 1893.

1893.	C.G.S. measure.	
	X or Horizontal force.	Y or Vertical force.
January	0·18447	0·43621
February	0·18457	0·43662
March	0·18438	0·43688
April	0·18467	0·43682
May	0·18470	0·43675
June	0·18482	0·43696
July	0·18466	0·43683
August	0·18460	0·43679
September	0·18434	0·43653
October	0·18445	0·43658
November	0·18451	0·43644
December	0·18444	0·43628
Means	0·18455	0·43664

Table VI.—Observations of Magnetic Inclination. Falmouth Observatory, 1893.

Month.		Mean.
January	28	67° 3′·7
	30	67 5·0
	31	67 5·5
		67 4·7
February	25	67 3·5
	27	67 6·2
	28	67 5·7
		67 5·1
March	25	67 6·2
	28	67 8·0
		67 7·1
April	15	67 4·1
	28	67 5·6
	29	67 5·3
		67 5·0
May	25	67 5·1
	26	67 2·9
	27	67 5·8
		67 4·6
June	27	67 3·8
	28	67 3·9
	29	67 5·4
		67 4·4

Month.		Mean.
July	28	67° 5′·8
	29	67 4·4
	31	67 5·0
		67 5·1
August	27	67 5·7
	28	67 5·2
		67 5·4
September	26	67 8·5
	27	67 6·1
	28	67 4·7
		67 6·4
October	28	67 5·4
	30	67 6·5
	31	67 5·6
		67 5·8
November	28	67 5·1
	29	67 5·0
	30	67 5·0
		67 5·0
December	27	67 4·7
	28	67 5·9
	29	67 4·3
		67 5·0

OBITUARY NOTICES OF FELLOWS DECEASED.

Frederick Le Gros Clark, F.R.S., F.R.C.S.Eng., who died on the 19th July, 1892, after a brief illness, at the ripe age of 81, was born on the 7th February, 1811, in Mincing Lane, the youngest of nine children of a city merchant. His early years were spent in the city. In 1822 he went to reside as a pupil with the Rev. Ford Richardson, at Iron Acton, in Gloucestershire, where he remained four years. Here he received a very excellent education. He had always expressed a desire to become a Surgeon; but his father, before deciding, consulted his friend, Mr. Benjamin Travers, then the distinguished Senior Surgeon to St. Thomas's Hospital, who allowed the son to have the run of the hospital for a couple of months, at the end of which time his father gave him the choice of entering his own counting house or of being apprenticed to Mr. Travers. In February, 1827, at the age of 16, he was apprenticed, and at once began his hospital career. He appears to have been an industrious and distinguished pupil, for in 1830 he obtained the Cheselden Medal, and in the same year was appointed Assistant Demonstrator of Anatomy, under Mr. Tyrrell. He spent the summer session of this year in Dublin. In 1833 he passed his examination at the Royal College of Surgeons. The summer session of that year he spent in Paris, that of 1834 in Edinburgh, that of 1835 in Berlin; and in 1836 he passed three months at Göttingen. In 1837 he took rooms near the hospital and started in practice. He continued working at the hospital, and teaching anatomy; and in 1842, when the Hospital Medical School was remodelled, he was elected Lecturer on Descriptive and Surgical Anatomy; and in 1843, on Mr. Tyrrell's death, he was appointed Assistant Surgeon. He then removed to Finsbury Square. In 1847 he was elected Surgical Secretary to the Medico-Chirurgical Society, and in the following year moved to Spring Gardens. In 1853 he was appointed full Surgeon to the hospital; and increasing engagements compelled him to retire from the Chair of Anatomy in 1854, though still retaining the lectures on Regional and Surgical Anatomy. In 1858 he removed to St. Thomas's Street, at the request of the Governors, and in 1860 became Lecturer on Surgery, an appointment which he held down to his retirement from the hospital. In 1864 he was appointed Examiner in Surgery to the Royal College of

Physicians for two years; and in July of the same year was elected a Member of the Council of the Royal College of Surgeons, and was at the head of the poll. In 1866 he was appointed Examiner in Surgery to the University of London for a period of five years; in 1867 Professor of Human Anatomy and Surgery to the College of Surgeons; in 1868 Hunterian Professor of Surgery and Pathology; and Examiner at the College in 1870. In 1872 he was appointed Vice-President of the College, and in 1874 President, giving the Hunterian Oration on the 13th February, 1875, the forty-eighth anniversary of his apprenticeship to the college. In 1872 he was elected a Fellow of the Royal Society. In 1873 he retired from the hospital, having retained office for a year or two at the special request of the Governors. In 1877 he gave up practice, and took up his residence permanently at Sevenoaks; and in 1879 he retired from the Council of the Royal College of Surgeons. But even after his retirement he remained a busy and active man. He still continued Consulting Surgeon to the South Eastern Railway Company; he was always ready to give professional assistance to his neighbours; he was Consulting Surgeon to and an active Governor of the Hospital with which he had been so long connected, took great interest in the welfare and progress of the Medical School, attended all anniversary and other important or interesting meetings, taking part in their proceedings; and he retained his connexion with the Salters Company, of which he had been twice master, at twenty years' interval. It should be added, that in addition to other duties he was for some years Surgeon to the Magdalen Hospital and to the London Female Penitentiary, and Consulting Surgeon to the Surrey County and Great Northern Hospitals.

Mr. Le Gros Clark was in many respects a remarkable man. In the first place he had striking physical endowments; he was tall and well made, spare but very muscular, singularly handsome, with dark curly hair and whiskers, dark grey eyes and bushy eyebrows, and well-formed features; and was remarkably dignified and gentlemanly in appearance and demeanour. As a young man he was a great athlete, excelling especially in rowing, boxing, and riding, and he retained this activity of body to the last. He was hardly what one would term a genial man; but he was a man of the highest character, he was absolutely unselfish and unself seeking; whatever he undertook to do he did with all his might, he was perfectly truthful and trustworthy, and always kind and considerate for others, and a warm and appreciative friend. He was a devout Christian, and member of the Church of England. As a surgeon and a teacher he was excellent. He was a thorough anatomist, and an admirable lecturer on anatomy. He had had a wide experience as a surgeon, and was admirably well up in the subject; he was conscientiously attentive to his patients,

and neglected nothing for their benefit, and he was a faultless operator. As a clinical teacher he was admirable. He was not, and did not aim at being, a speaker of commanding eloquence; nevertheless he was a most excellent and ready lecturer and speaker. His manner was always quiet and gentlemanly; his language was always simple and well chosen; and his matter was always appropriate to the occasion. He was consequently not only a clear and attractive lecturer, but he was a clear and attractive speaker on all festive and other occasions. Although he was a scientific surgeon and anatomist he did not do much original scientific work. He wrote many papers on points in anatomy and surgery that interested him; he lectured (as before stated) at the Royal College of Surgeons. He delivered three introductory addresses at St. Thomas's Hospital. While president he delivered the Hunterian Oration at the College of Surgeons, an address which was philosophical and full of thought. He contributed a paper on 'The Mechanism of Respiration' to the 'Proceedings of the Royal Society' (vol. 20, 1872). In 1836 he published a work on the anatomy and physiology of the nervous system; in 1847 and 1853 he translated two volumes of Dupuytren's 'Leçons Orales' for the Sydenham Society; he published 'Lectures on Surgical Diagnosis of Visceral Lesions' in 1870; and 'Outlines of Surgery,' of which a second edition appeared in 1872; Paley's 'Natural Theology,' edited for the S.P.C.K. in 1875; a little manual of physiology for the same society in 1883; several articles on anatomy and physiology in the 'Encyclopædia Metropolitana' about 1840; several papers in the 'Medico-Chirurgical Transactions;' critical articles in the 'British and Foreign Quarterly,' and he made a few contributions of cases to the medical papers. Lastly, he published a collection of 'Miscellaneous Essays,' which had already appeared in various periodicals, &c.

J. S. B.

INDEX TO VOL. LVI.

ERRATA.

Page 12, line 8, after the word "grams" *insert* "of ballistite."
" 252, " 10, *for* "charge" *read* "charged."

END OF FIFTY-SIXTH VOLUME.

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